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AN EXPERIMENTAL STUDY OF NAMING

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The nature of the relationship between an object and its name has interested thinkers since early times. At a period as early as that of the Greeks, there was much speculation about this general problem, the Sophists denying the possibility of any objective interconnection between object and name, and the Stoics affirming the objective validity of words.[1] A more particular question has been formulated by Jespersen as follows: "Is there a natural interconnection between sound and meaning? Do words acquire their meanings through sound-symbolism?"[2] Even in a work dating from the fourth century B.C., Plato's dialogue *Kratylos,* we note an attempt to find in the sounds of certain letters of the alphabet bases for names, *e.g., rho* and *iota* are said to express motion, and *lambda* smoothness.[3] Since linguistic historians in modern times have been occupied chiefly with words which have already undergone extensive phonetic and semantic change, it is intelligible that many of them should have rejected entirely the possibility of sound-symbolism in names.

Perhaps no linguist has so clearly represented the view that all names are conventional as William Dwight Whitney, who writes: "Inner and essential connection between idea and word, whereby the mind which conceives the one at once apprehends and produces the other, there is none, in any language upon earth. Every existing form of human speech is a body of arbitrary and conventional signs for thought."[4] In commenting on this view, Jesperson says, "There is no denying, however, that there are words which we feel instinctively to be adequate to express the ideas they stand for, and others the sounds of which are felt to be more or less incongruous with their signification." Jesperson lists several classes of sounds adapted

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[1] E. Cassirer, *Philosophie der symbolischen Formen: I. Die Sprache,* 1923, 132-133.
[2] O. Jespersen, *Language: Its Nature, Development and Origin,* 1922, 396.
[3] B. Jowett, *The Dialogues of Plato translated into English,* etc., 1, 1892, 372-373.
[4] W. D. Whitney, *Language and the Study of Language,* 1867, 32.

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to express certain notions symbolically, *e.g.* the vowel *i* (pronounced as in *little* or like the *ee* in *wee*) "is particularly appropriate to express what is small, weak, insignificant, or, on the other hand, refined and dainty."[5]

Von Hornbostel has applied his general doctrine of the 'unity of the senses' to the problem of name-symbolism. The general principle he has formulated as follows: "Das Wesentliche des Sinnlich-Anschaulichen liegt nicht in dem, was die Sinne trennt, sondern in dem, was sie eint—eint unter sich, mit dem ganzen, auch nicht-sinnlichen Erleben in uns, und mit all dem draussen, was es zu erleben gibt."[6] He has made his position very clear in his study of brightness, in which he maintains that "brightness is not only an analogous but an identical aspect of the phenomena of the various sensory fields."[7] Certain psychophysical studies, which unfortunately are not reported fully by Von Hornbostel, suggest that it is possible to match in brightness sense-impressions of disparate senses (smell, audition, vision).[8]

Von Hornbostel has approached the problem of naming directly in another article on sound and meaning.[9] He maintains that the meaning of a speech-utterance is dependent at any time upon the inner and external situation of the organism. "Der Sinn bestimmt den Laut."[10] Even more striking is the statement, not intended to be merely figurative, that "Sprache ist tönender Sinn."[11] He concedes that the behavior of the 'other individual' is partly determined by *his* inner situation and *our* perception of his behavior by *our* inner situation, so that the original meaning of an expression might be transformed beyond recognition. Two facts, however, must be considered: (1) in primitive life individual differences in past experience are slight; and (2) in primitive life creatures live in a narrow spatial and temporal present. Expressed in extreme form, the theory is that such primitive individuals have a common experience. The meaning of a sound-expression, which may be one aspect of such a common experience, is the meaning of the common situation. "Der Laut ist also dem Sinn ursprünglich vollständig adäquat." No experience *(Erfahren)* is needed to understand what is heard—for the meaning is heard just as it may be seen, felt or expressed. Linguistic proto-creation *(Urschöpfung)* is necessarily rare in modern man born in a social milieu in which language is already in use, yet Von Hornbostel believes that even today we may observe such proto-creation of names, particularly of those—such as nick-names—used to characterize personalities.[12] After stating his theory, Von Hornbostel turns to a consideration of certain features of extant languages. He believes that the essential key to a study of sound-expressions lies in speech-melody, of which the following are the important aspects.

(1) Intensity and rhythm. For example, Indogermanic disyllabic roots with verbal significance have, with iambic accent, a strong, transitive, active meaning; with trochaic accent, a weak, passive, intransitive meaning.

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[5] Jespersen, *op. cit.*, 398 ff.

[6] E. M. von Hornbostel, Die Einheit der Sinne, *Melos*, 4, 1924, 290-297.

[7] Von Hornbostel, Über Geruchshelligkeit, *Arch. f. d. ges. Physiol.*, 227, 1931, 517-538.

[8] Also cf. L. Székely, Über den Aufbau der Sinnesfunktionen, *Zsch. f. Psychol.*, 127, 1932, 227-264. N. E. Cohen has recently published some quantitative findings which "indicate too great a discrepancy to render the [Von Hornbostel's] hypothesis of identity plausible" (Equivalence of brightness across modalities, this JOURNAL, 46, 1934, 117-119).

[9] Von Hornbostel, Laut und Sinn, *Festschrift Meinhof: Sprachwissenschaftliche und andere Studien*, 1927, 329-348.

[10] *Ibid.*, 329-330. [11] *Ibid.*, 331. [12] *Ibid.*, 331-332.

(2) Duration (indicating length and shortness).
(3) Pitch-movement (indicating rising and falling).
(4) Pitch of the voice (high sounds having the meaning of smallness or thinness, low sounds the meaning of bigness or thickness).
(5) Vowel-brightness. For example, the Bantu *-tetema* means "to quiver"; while *-tutuma* in the same language means "to thunder."
(6) Characteristics of the consonants. Brightness, shortness, sharpness and hardness of consonants indicate strength or activity. Dullness, length, mildness and softness indicate weakness or passivity.[13]

Von Hornbostel considers in detail the dullest linguistic sounds—*m, mb, omb*—as they appear in words in the Indogermanic and Semitic languages and also in Bantu and Hungarian. No perfect parallelism between sound and meaning can be expected in living languages, yet words containing *m, mb,* and *omb* are likely to have such meanings as these: *dull, dark, heavy, thick, to enclose, fat, damp, mother, blunt, hollow, deep, large.*[14]

Westermann has studied the 'phonetic symbolism' of certain African languages.[15] He limits himself to four contrasts which may be noted in the sounds of the languages which he has studied and furthermore has considered mainly only those African words which stand in meaning near the world of sense-perception. He arrives at the correlations shown in Table I. He expresses the caution that "this grouping must naturally be applied not mechanically but in accordance with the total course of speech."[16] The sounds considered by Von Hornbostel—*m, mb, omb*—fall in Westermann's B-series, and the characterizations given by Von Hornbostel agree closely with those found by Westermann in other languages.

Another series of studies bearing upon the problem of naming was initiated by Claparède and Kollarits.[17] Their problem was to determine the nature of the psychological responses to proper names of unknown persons or places. Experimental methods were applied in a group of studies from the Cornell laboratory. In the first of these, English[18] presented various names of individuals unknown to her subjects and asked for descriptions of the resulting experiences. Claparède had made the suggestion that the 'physiognomy' of a proper name may play a rôle in determining the meaning apprehended in it by the observer. In English's study only a few such physiognomic cases appeared, *e.g. Klemm* was reported to be a "sharp word," and *Ponsonby* to be a "heavy word." In another series, English presented nonsense names and called for descriptions of individuals who might well bear such names. The *O*s were also shown pictures of individuals for whom names were to be chosen. "If we take the 500 reports as a whole, the results are negative. There is no constant or uniform tendency among these observers either to imagine a similar type of individual for the same name, or to furnish a similar type of name for the same picture."[19] The results of this series suggested also that the 'physiognomy' of the name was a relatively unimportant factor.

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[13] *Ibid.,* 337. [14] *Ibid.,* 339.
[15] D. Westermann, Laut, Ton und Sinn in Westafrikanischen Sudansprachen, *Festschrift Meinhof,* 1927, 315-326.
[16] *Op. cit.,* 328.
[17] E. Claparède, Sur la représentation des personnes inconnues et des lapsus linguae, *Arch. de Psychol.,* 14, 1914, 301-304; K. Kollarits, Observations de psychologie quotidienne: I. Sur les images qui accompagnent la représentation des individus et des lieux inconnus, *ibid.,* 225-240.
[18] G. English, On the psychological response to unknown proper names, this JOURNAL, 27, 1916, 430-434.
[19] *Op. cit.,* 432.

TABLE I

WESTERMANN'S CORRELATION BETWEEN SOUND AND MEANING

	Series A	*Series B*
Sound	High tone	Low tone
	Short vowel (quantity)	Long vowel (quantity)
	Bright vowel (*i*, *e*, *a*)	Dull vowel (*u*, *o*)
	Voiceless consonant	Voiced consonant
Meaning	Small, fine, tense, hard, solid	Large, voluminous, heavy, shapeless, soft loose, etc.
	Bright	Dull
	Quick	Slow
	Sharp, pleasant taste	Tasteless, insipid
	Pleasant smell	Unpleasant smell
	Intensive color	Dull color
	Energetic, fresh, exact	Dull, heavy, blunt, chaotic

Alspach used only one *O*, who had also taken part in English's experiment.[20] Fifty meaningless words were read in turn to the *O*, who was instructed to describe a person who might bear each name and to give introspective comments. In some cases, the name suggested a particular nationality. In others, a similar name was suggested, and a person bearing this latter name was described. Auditory-verbal associations were sometimes effective. *Gronch*, for example meant "a kind of surly, grouchy, piggish, selfish, self-satisfied beggar; has a thick, heavy mustache; grumbles. Thought of *grouch* and *grunts*." With 25% of English's words and 35% of Alspach's, the *O* was unaware of any such sensible associations but indicated that the mere sound of the word was the determining factor. Alspach concludes that "the sound of the name was effective in a much larger percentage of cases than former work would lead us to expect."[21] The words were read to the same *O* after an interval of 15 mo. The correspondence between the earlier and later reports of the *O* appeared to be greater than chance would allow.

In 1922, Fischer[22] published a series of two articles dealing with the psychological nature of names. He assigned arbitrary nonsense names to nonsense drawings and photographs of individuals and called for reports indicating the experienced relationships between words and objects. Fischer observed, among other things, that the word did not always become the 'name' of the object. When the word appeared as a name, a very particular experience was always involved: with the hearing or pronunciation of the word, the *O* 'meant' the object or was directed toward it.

Two years later Usnadze reported the results of another study of naming *(Namengebung)*.[23] Since our own experiments were undertaken initially as a check upon Usnadze's, we must report the latter in some detail. Uznadze drew 6 nonsense figures; we can only guess at their nature, since he has published no examples. Each

[20] E. M. Alspach, On the psychological response to unknown proper names, this JOURNAL, 28, 1917, 436-443.

[21] *Ibid.*, 439.

[22] S. Fischer, Über das Entstehen und Verstehen von Namen, *Arch. f. d. ges. Psychol.*, 42, 1922, 335-368; 43, 1922, 32-63.

[23] D. Usnadze, Ein experimenteller Beitrag zum Problem der psychologischen Grundlagen der Namengebung, *Psychol. Forsch.*, 5, 1924, 24-43.

figure was exposed for 5 sec., the *O* having the task of discovering the most suitable name for it from a list of meaningless sound-complexes. The *O*s, numbering 10 in all, were also asked to report their experiences. Usnadze does not indicate the number of sound-complexes presented with each figure; 42 sound-complexes were used in all.

The reports of Usnadze's *O*s indicate that the selected word is not applied arbitrarily to the object, one or more of certain particular factors being involved. Of these factors 4 may be distinguished.

(a) Associative assimilation. The sound-complex is associated with a familiar word which expresses the significance of the apperceived object.

(b) Configurative relationship. O experiences an agreement *(Zusammenpassen)* between the perceived form or configuration of the sound-complex and that of the object.

(c) Affective relationship. O experiences an agreement between the feeling aroused by the figure and that aroused by the word chosen as its name.

(d) Relationship in mental substructure (Tiefenschichtfaktor). O has, accompanying the perception of the two components, a 'general impression' serving as a basis for the naming-function.

Factors *b, c,* and *d* may be called the objective, material or intrinsic *(sachliche)* factors in naming and are thus all set off against the first or associative factor.

Usnadze is convinced that the various factors are not all of equal importance in naming. The associative factor in particular does not seem to be such that it will give the 'right word' with any satisfactory degree of stability. After a "sufficiently long interval," Usnadze re-presented the figures to some of the *O*s and asked them to name the figures as they had done before (*Series A*). If the *O* could not recall the word, he was again shown the list of words and asked to pick out the name which he had selected before (*Series B*). In Series A, those complexes which had been chosen on the basis of the associative factor were often retained better than the others, but usually only a part of the sound-complex, or perhaps only the basis for the assimilation, was recalled. In Series B, the *O* was often led astray in cases where he had given associative names and now often chose other words as names for the same figures. The results were quite different in the cases in which the intrinsic factors (*b, c, d*) had been reported. In Series A, memory was usually quite unfaithful; in such instances the *O* could not even give individual parts of the names he had previously chosen. In Series B, on the other hand, the *O* typically did not 'know' which word he had chosen before (when the word had been chosen on an intrinsic basis) but nevertheless chose the same word the second time in almost all cases. Usnadze concludes from these data and from spontaneous comments by the *O*s that the intrinsic factors have greater importance in founding language than has the associative factor. This conclusion led him to the expectation that the various *O*s would show considerable agreement in the names which they chose for the six figures. As we have indicated, Usnadze used altogether 42 sound-complexes and presumably seven with each of the six figures. Each figure was presented to 8 or 9 *O*s. Of the 42 sound-complexes, only 29 were chosen as names. In the case of the first figure, 45% of the *O*s agreed upon one and the same name; the corresponding percentages for the other figures were 40%, 25%, 40%, 38%, and 40%. "Bei der zufälligen Herstellung und ziemlich grossen Zahl der auswählenden Lautkomplexe ist

also der Prozentsatz der Überstimmung der Versuchspersonen beinahe unerwartet gross ausgefallen."[24] Usnadze found further that the associative factor had in only one single case produced an agreement in names; the other cases were distributed almost uniformly among the objective or intrinsic factors.

Usnadze's general conclusion is that the associative factor functions only as a substitutive factor, the others being of greater significance in naming. "Das Wort würde nicht zufällig assoziations- und blinderfahrungsmässig an das beliebige Objekt herantreten und auf diese Weise als Glied zufälliger Verkettung funktionieren, sondern es scheinen bestimmte sachliche Gesetzmässigkeiten entscheidend, denen die menschliche Seele auch auf dem Gebiete der Namengebung folgt."[25]

Usnadze's study is subject to attack on several grounds. (1) He has generalized from far too few cases. It seems certain from his own statements that there could not have been more than 55 (and perhaps as few as 49) observations in the main series. (2) Each *O* observed in so few cases that he was probably quite untrained in this particular field of investigation. (3) Usnadze makes the tacit assumption that the true bases for the selection of names will be indicated in the verbal reports. He does not consider the possibility that the real bases or grounds may lie partly or entirely outside of the level of report, nor does he refer at all to 'rationalized' reports, of which it seems likely there may have been examples. (4) Usnadze's statistical findings hardly authorize the force of certain of his conclusions. His vagueness about the number of sound-complexes presented with each figure, etc., makes a systematic checking of his data impossible. (5) Usnadze reports that the non-associative factors underlay all but one of the agreements in names; but he does not inform us whether the *contents* of the reports made by the several *O*s selecting the same sound-complex show any agreement.

Köhler has recently contended that "there are some similarities between the experiences we have through different sense organs."[26] He refers in a foot-note to Usnadze's study. Köhler has constructed two figures and two nonsense sound-complexes and asserts that any individual will probably be able to decide with ease how the names and figures should be paired."[27] We report below (*Series I*) some group-experiments in which we used Köhler's figures and words. Perhaps motivated by Köhler's discussion, Tsuru and Fries have described a phenomenon which "challenges any behavioristic and associationistic explanation."[28] Twenty-five pairs of English words (*e.g.* large—small) are presented, each pair coupled with a pair of Japanese words having these same meanings. The writers give no detailed results but state that in about 75% of the cases an average subject is able to 'guess' which of the two Japanese words belongs in meaning with the first English word and which with the second. We are offered a provisory explanation rooted in the theory that "meanings, psychologically, are properties of natural organized wholes or processes."

There have recently appeared in this country certain significant studies in which attempts have been made to attack problems suggested by certain particular 'phonetic

[24] *Ibid.*, 42.
[25] *Ibid.*, 42.
[26] W. Köhler, *Gestalt Psychology*, 1929, 242-243.
[27] We have reproduced Köhler's figures and words in Fig. 1.
[28] S. Tsuru and H. S. Fries, A problem in meaning, *J. Gen. Psychol.*, 8, 1933, 281-284.

symbolisms' which appear to be involved in language. Sapir,[29] who initiated this series of investigations, undertook to ascertain "if there tends to be a feeling of the symbolic magnitude value of certain differences in vowels and consonants, regardless of the particular associations due to the presence of these vowels in meaningful words in the language of the speaker." His experiments, which were based essentially upon a method of paired comparison, demonstrate that such 'feelings' are experienced, and that his subjects agree fairly well in their scales of symbolic size.

Newman[30] has carried Sapir's work further, particularly through the use of a statistical method devised by Thurstone.[31] In his first experiments, Newman used 100 pairs of nonsense-words, the two in each pair differing only in their vowels. The words in certain pairs were arbitrarily defined as nouns, others as verbal subjects, others as adjectives, etc. Each of the 606 *O*s had the task of selecting the 'larger' word in each pair. The vowels used in the words tended to vary fairly consistently in their symbolic magnitude, *i* (as in Fr. *fini*) being the 'smallest' and *o* (as in Fr. *note*) the largest. In his second experiment, Newman studied two particular symbolisms: large *vs.* small, and dark *vs.* bright. He constructed 113 word-pairs contrasting 9 vowels and 15 consonants and presented each contrasting pair of speech-sounds once to each *O*. The vowel *a* (as in *tall*) appeared to be the 'largest' of those used, and *i* (as in *fit*) the 'smallest.' The 'largest' and 'smallest' of the consonantal sounds employed were *br* and *p* respectively. The orders for the vowels and consonants on a brightness-darkness scale were somewhat different.

To determine whether such phonetic symbolisms as he discovered play an actual part in the English language, Newman made a careful study of the words given in Roget's *Thesaurus* under the rubrics of *greatness, smallness, size* and *littleness*. He concludes that "the phonetic content of English words takes practically no account of magnitude-symbolism."[32] The implication of Newman's finding stands in sharp contrast to the conclusions of Von Hornbostel and Westermann. Although Newman's statistical refinements increase the cogency of his argument, it should be noted that the German investigators were working particularly with languages less evolved than English, and that thus there may be no basic discrepancy between their data and those of Newman.

In a study undertaken as a check upon the investigations of Sapir and Newman, Bentley and Varon[33] employed four different methods. The results of their first experiment, in which a method of 'free association' was utilized, suggest that "such sounds as we have employed do not *spontaneously* lead to other sounds (under the common conditions of 'free association') which denote or hint at spatial magnitude or its qualifications."[34] In the second method, the experimenter read a nonsense-syllable, the *O* being instructed to indicate the degree to which it represented some category (*size, angularity, foolishness,* etc.) or its opposite. In 450 of the 1,500

[29] E. Sapir, A study in phonetic symbolism, *J. Exper. Psychol.*, 12, 1929, 225-239.
[30] S. S. Newman, Further experiments in phonetic symbolism, this JOURNAL, 45, 1933, 53-75.
[31] Cf. L. L. Thurstone, A law of comparative judgment, *Psychol. Rev.*, 34, 1927, 273-286; An experimental study of nationality preferences, *J. Gen. Psychol.*, 1, 1928, 405-425.
[32] Newman, *op. cit.*, 74.
[33] M. Bentley and E. Varon, An accessory study of "phonetic symbolism," this JOURNAL, 45, 1933, 76-86.
[34] *Ibid.*, 78.

reports, the *O*s mentioned some sort of relationship between the category-word and the non-linguistic sound. In the third method, five categories (*foolish, angular, liquid, large, strong*) were employed. The non-linguistic sounds were presented in pairs, the *O* being instructed to report which (if either) of these sounds more closely approximated the meaning of the category. In the 300 pairs of sounds used, there was some relation to the category 165 times. There were, however, only 7 cases of very marked relationship between sound and category. Comments by all the *O*s seemed to bear out the suspicion that the relation was usually forced rather than spontaneous. The fourth experiment was a group-experiment with 26 *O*s. Two non-linguistic sounds, one containing an *a*-sound and the other an *i*-sound but otherwise identical, were given an arbitrary meaning, such as *table,* and the *O*s were instructed to report which of these two sounds represented, say, a larger table. It was discovered that *a*-sounds were larger than *i*-sounds, with an approximate ratio of 4:1. *A*-sounds likewise tended to be 'rounder' and 'softer' than *i*-sounds, although the correlations were imperfect. The writers emphasize the point that these positive reports appeared only when some degree in a scale was suggested and prescribed.

The general conclusion reached by Bentley and Varon is that "certain tonal, vocalic, and consonantal characteristics, which likewise bear a fixed relation to physical sources of sound and also to the human productive mechanisms . . . are so well graduated as to be usable when forced by artifice into the representation of size and of various other concepts and categories as well. Nevertheless, there seems to be insufficient evidence that these graded attributes of sound carry in their own right (so to say) a symbolic reference."[35] Supplementary experiments showed that untrained *O*s were able to distinguish differences in largeness, softness, lightness, etc., not only between pairs of articulated nonsense-sounds but also between pairs of tones and noises, with nothing like symbolic reference being involved.

The Experiments

By the use of methods patterned primarily after Usnadze's, we attempted to face once more the question whether there is an intrinsic connection between an object and a name selected for it. The phrase 'intrinsic connection' is, however, so ambiguous that we must make a few important distinctions. To say that there is such a relationship between an object and its name may mean (1) that there is an ultimate ontological connection between the two; (2) that different individuals tend to agree upon one word or sound-complex as the best name for the object; (3) that one individual tends to select at different times the same name for the object; (4) that the connection is unlearned or is independent of the particular specific training of the individual; (5) that there is an experienced interconnection or relationship between object and name; (6) that the individual is able to find 'reasons' for the suitability of a particular name for the object; (7) that there are real objective bases determining the choice of the particular name for the object. Whether or not there may be in-

[35] *Ibid.*, 85.

trinsic connections of the first sort cannot be determined by scientific methods, for the question is essentially philosophical. The fourth meaning of 'intrinsic connection' also lies outside of our present study, not because it is in principle inaccessible to experimentation, but because we have used as observers only English-speaking adults. Similar studies with children and with individuals of diverse linguistic heritages would be practicable and enlightening. We are left, then, with five major questions: (1) Do different individuals tend to agree upon the same word as the most appropriate name for a given object? (2) Is there a relatively stable relationship between name and object, in the sense than an *O* will, at different times, tend to select the same word as the name for the object? (3) Do *O*s experience non-arbitrary relationships between object and name? (4) Do *O*s tend to find reasons contributing to the suitability of the selected name? (5) Are there real objective bases, possibly unreported or even unreportable, which determine the choice of one sound-complex rather than another as the best name for a given object?

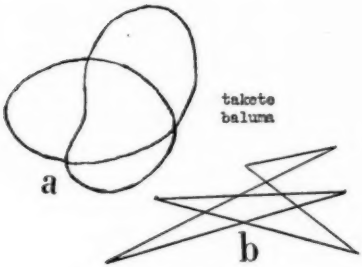
In most of the series we have used only 5 *O*s, all of whom have had some psychological training, and have required full verbal commentaries. Since we have not, except in Series I, resorted to the method of group-experimentation, our observations are fewer in number than those in, say, Newman's investigation. We have, on the other hand, been able to collect data much more extensive than Usnadze's.

One point in our general method demands some consideration: the fact that the words were presented visually. It might appear preferable for the words to be presented to the ear rather than to the eye, but several counter-arguments may be raised. (a) Usnadze's experiments, which we were attempting to check, involved visual presentation of the words. (b) When the words are presented visually, there is a greater possibility of survey and comparison by the *O*s. (c) A somewhat shorter exposition-time may be used with words presented visually. (d) There are fewer difficulties and complications of a purely technical sort in the visual presentation. (e) It may also be said that basically a word is a visual-auditory-kinaesthetic complex, and that it tends to retain a certain invariance or constancy regardless of the sensory avenue through which it is apprehended.

Series A

Our problem in the initial series of experiments was to study the procedure by which an *O* selects a name for a nonsense-object from a series of nonsense sound-complexes when he is instructed that later he will be called upon to recall the name.

Procedure. We made 14 nonsense-drawings, each consisting of 4 strokes of the pen; a 'stroke' was either a straight line, an arc of a circle, or a complete circle. The drawings were made as distinctive as possible. We also constructed 84 nonsense-words as follows: 28 consisted of two consonants with a vowel between them, *e.g. teb;* 28 of three consonants and two vowels, the consonants and vowels appearing



takete
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Köhler's Figures

lepeg
cuxim
buv
zuquidur
rux
wumupun

Series A 1

mizif
gagagon
paj
cawir
faquugup
har

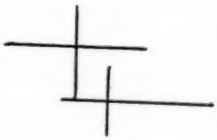
Series A 11

woniyam
seyahal
lobor
ler
quar
vuwol

Series B 21

dajiquil
yev
calaheg
rogut
nom
lenav

Series B 41



nimep
kosumop
zitix
cuk
gak
lavikub

Series B 69

gakomok
rililin
maxob

Series F Er 11

gakomok
rililin
maxob

Series H Er 11

FIG. 1. EXAMPLES OF WORDS AND FIGURES

alternately, *e.g. wenup;* and 28 of four consonants of four consonants and three vowels, the consonants and vowels appearing alternately, *e.g. sidakob.* In forming these words,[36] we used all the letters of the English alphabet; the 21 consonants were used with as equal frequency as possible, and likewise the 5 vowels. We always inserted a *u* after each *q* and never used *q* in the terminal position. With a few other slight reservations, the words were constructed in a purely haphazard manner. Each figure was drawn in black India ink on a white card 5 x 8 in. and at its right side 6 words (two of each of the three types, arranged in random order) were typewritten in a vertical column. An example of a figure and its appertaining words is given in Fig. 1.

The *O* sat in a low chair facing a vertical gray screen (28 x 22 in.) in the center of which was an opening behind which the card was placed. A simple system of metal slides controlled by electro-magnets was used to govern the length of the exposition-time (10 sec.). The cards, which were semi-translucent, were illuminated from behind the exposure-apparatus by a 50-watt electric bulb in a reflector.

The 5 *Os*[37] were given some preliminary practice in pronouncing words similar in construction to those used in the experiment. They were instructed to pronounce the vowels as follows: *a* as in *father; e* as in *they; i* as in *machine; o* as in *pole;* and *u* like the *oo* in *food.* The consonants *c* and *g* were to be pronounced as in *cat* and *dog* respectively. The other sounds were to be given their ordinary English pronunciations. The *Os* were instructed further that the first syllable of each word was to be accented. The *Os* followed surprisingly well these instructions regarding pronunciation.[38]

At the beginning of each observation-period, *O* was given the following main set of instructions:

After the signal 'Ready—now,' a visual figure and 6 nonsense-words will be exposed for 10 sec. Find that one word among the 6 which best fits the figure or is the best name for it. After the end of the exposure, report first the word you have chosen and then give as full a commentary as possible about the entire experience. Later you will be asked to recall the name when the figure is presented alone.

E used a stop-watch to control the exposition-time; more exact regulation seemed entirely unnecessary. The *Os* were able to give their verbal reports fairly rapidly, since *E* recorded them in shorthand.

Each *O* named 7 of the figures in each experimental hour. After each such subseries, *O* was given a rest-period of 5 min.; this interval was spent in general conversation. Then he was given these instructions:

At the signal 'Ready—now,' I shall expose for 5 sec. a figure which you have already seen today. After the exposure report a name for the figure—either the one you selected before if possible, or if not a nonsense-word which would fit the figure.

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[36] For the sake of brevity, we shall often speak of 'words' in contexts where 'nonsense sound-complexes' would be the more exact term.

[37] In this and most of the other series the *Os* were Mr. A. Douglas Glanville (*Gl*), Mr. Merrill F. Roff (*Ro*), Miss Charlotte A. Stewart (*St*), Miss Eleanor Lowenstein (*Lo*), and Miss Margaret B. Erb (*Er*). *Gl* was an assistant in the Department of Psychology and had had extensive training and practice in psychological observation. *Ro,* who was an assistant in the Department, and *St, Er* and *Lo,* who were graduate students, had all had some training in observation.

[38] There was a slight tendency for some of the vowels to be shortened, *eg.* for *i* to approach the *i* in *fit* or *u* to approach the *u* in *pull.*

The figures were then exposed, in random order, for 5 sec. each, the typewritten words on the cards being covered.

Results. In the very first part of the experiment, 4 of the 5 *O*s (all except *Gl*) were somewhat confused by the instructions and found little meaning in the problem confronting them.

"It's all very vague. There doesn't seem to be any method of telling which word should belong with that figure. It is simply a matter of picking out one of the words, one easier to remember" (*Er* 1).

Very shortly, however, all the *O*s recovered from this initial confusion.

The *O*s tended to emphasize in their reports their 'reasons' for having chosen the particular names. The 'reasons' fall quite easily and naturally into the following classes:

(1) The figure is assimilated to some familiar object whose name might well be a familiar word suggested by one of the nonsense-words. This nonsense-word then becomes the name for the object. This type of 'reason' may be identified with Usnadze's 'associative assimilation."

"The figure might be taken roughly as a honey-comb, which is similar in sound to *honaxov*" (*Ro* 3). "The word *kiwagum* and the figure suggested *wigwam*" (*Lo* 4).

(2) The figure and one of the words are apprehended as having a particular common characteristic, *e.g.* complexity, angularity, or smoothness. In these cases, there is no appeal to associations with familiar objects or familiar words. The naming is reported as not depending upon *O*'s linguistic resources, and is in that sense more immediate than naming of the first type.

"I wanted something to represent the angles; the last two syllables represented the straight lines. It (*yobimax*) was better than the others" (*Lo* 3). "The figure itself aroused pronounced but rather vague empathic feelings which had something sharp about them and at the same time forces pulling in opposite directions. Before I had got completely through the list of words once, I felt that *mizif* was going to be the most satisfactory one there" (*Gl* 11).

(3) The *O* reports that the word chosen is a satisfactory name but can give no reasons. This is equivalent to Usnadze's factor of 'general impression.'

"Immediately I felt that *fezipap* was the most satisfactory one, but I found it impossible to give any reason for that choice" (*Gl* 8).

These three main types of report are in close agreement with three of the types distinguished by Usnadze. We found, in our 70 reports, no cases of purely 'emotional' or 'affective' fitting, although a few of the reports falling in the other groups indicated affective coloring. Many of the reports indicated that the *O* had either made no real choice of a name or had relied upon characteristics of the word having nothing to do with the problem, *e.g.* position of the word in the list, pleasantness of the word in its own right, familiarity of the word (quite apart from associations connecting it with the figure). These negative cases are important and numerous enough, in this and other series, to be distinguished as a fourth main group.

"My choice of *rux* was dictated partly by brevity and consequent ease and familiarity, partly by its position toward the end of the list, partly by the fact that I liked the sound of it . . . " (*Ro* 1). "There is no connection. None seems applicable" (*St* 13).

Of the 70 reports, 24 fell in the *associative* group; 20 in the group characterized by a matching of certain aspects or attributes of sound-complex and figure (*at-*

tributive group); 4 in the group characterized by unrationalized *general impression;* and 22 in the *negative* group in which there was no real choice, in any strict sense.

To indicate the extent of agreement between the *O*s in their choices of names, we introduce some simple and convenient symbols. The symbol A^5 will be used to refer to cases in which all 5 *O*s agree upon the same name for the given figure; A^3B^2, to cases in which 3 *O*s choose one word, the other 2 a second word; ABCDE, to cases in which all the *O*s choose different words; etc. In the first column of Table II are listed the various possible patterns of agreement and in the fourth the number of cases (out of 14) falling under each. An examination of the table reveals what at first glance might appear to be a striking agreement between the *O*s, *e.g.* in only one case out of the 14 did all the *O*s disagree. There were, however, no cases in which all 5 *O*s picked the same word. The data in this series are insufficient for statistical treatment, but for the sake of completeness we

TABLE II

AGREEMENTS AMONG *O*s IN SERIES A

Pattern of agreement	A priori probability	Expected frequency	Empirical frequency
A^5	0.00077	0.01	0
A^4B	0.01929	0.27	1
A^3B^2	0.03858	0.54	1
A^3BC	0.15432	2.16	1
A^2B^2C	0.23148	3.24	7
A^2BCD	0.46296	6.48	3
ABCDE	0.09259	1.30	1

have presented in the second column the theoretical or *a priori* probabilities of the occurrences of such events simply according to chance. These probabilities are easily determined by elementary principles.[39] In the third column we give the theoretical expected frequencies for 14 cases. Taken altogether, the data would suggest that the *O*s agree slightly more than chance would permit. We cannot conclude, of course, that the agreements are due entirely to factors intrinsic to the problem; purely adventitious factors may, and probably did, play some part. There was, for example, a much stronger tendency for the *O*s to select the first word in each list than any other; sometimes the *O*s reported position as the basis for 'choice' but more often they did not. There was also a greater tendency for the *O*s to select polysyllabic than monosyllabic words. This tendency is probably not a purely extraneous factor, since most of the figures were fairly complicated and, as some reports indicated, long words appeared, for that reason, to be more appropriate.

In summarizing the result of the recall-series, we may distinguish correct recalls (cases in which the *O* gave the word he had chosen in the original series), incorrect recalls (in which the *O* gave a word quite different from his original word), and semicorrect recalls (in which the word reported by the *O* contained half or more of the letters of the word originally chosen and in the same order).[40] The individual dif-

[39] For ease of analysis, we may think of 6-sided dice instead of cards with 6 words, and of five dice instead of 5 *O*s, and ask such questions as this: "What is the probability of getting the same number 5 times in throwing 5 dice?"

[40] Cf. Fischer, *op. cit.,* 342.

ferences between the *O*s were great; *Ro,* for example gave only one correct recall and 2 semi-correct recalls, and *St* gave 10 correct recalls, 4 semi-correct recalls and no incorrect recalls. A consideration of the results from all the *O*s shows that in cases where the reported bases for naming in the original series were associative, 10 of the 24 recalls were correct, 11 semi-correct, and 3 incorrect. In the 20 cases in which the original reported basis was attributive, there were 4 correct, 5 semi-correct, and 11 incorrect recalls. The 4 cases based, according to the reports, upon general impression produced 1 correct recall, 1 semi-correct recall, and 2 incorrect recalls. In the 22 'negative' cases, 5 recalls were correct, 1 semi-correct, and 16 incorrect. The percentage of correct recalls, and likewise of semi-correct recalls, was much larger in the associative group than in any other. Usnadze found in these cases a large number of what we have termed 'semi-correct recalls,' but did not, apparently, find as many examples of correct recall as we have.

We soon discontinued the method used in this series, since the instructions tended to make the *O* select the word which would be most easily remembered rather than the word which would be the best name for the object.

Series B

The second series of experiments were undertaken in order to escape the complications of memory which we noted above.

Procedure. We prepared, in accordance with the principles used in Series A, 100 cards each containing one nonsense-drawing and 6 nonsense-words.[41] The general procedure was the same as in the preceding series. The last sentence of the instruction, however, was omitted, and the *O*s were assured positively that there would be no recall-series similar to those in the earlier experiment.

Results. The Os' reports revealed at once two important facts. In the first place, the *O*s were able, in the great majority of cases, to select a name which was definitely better than the others. Secondly, the reports were often extraordinarily long and complex; only in rare cases was the *O* able to characterize his experience briefly and simply. The following report is typical:

"The figure amazed me. This amazement was due to the regularity of the figure. I sat here for some time staring at the figure and wondering what it all meant. I repeated to myself several times, 'There's a right-angled triangle with those two concentric arcs within it. What does it all mean?' I turned to the list of words without the slightest idea of what sort of word I was looking for. In other words, there was no dominant feature or characteristic of the figure which I expected to be expressed by some of the words. I went through the list of words again and again without finding any one word that was appropriate to the figure. Finally, I hesitated over the word which I gave you (*buzobud*) and for some reason or other I tried to find in that one word something which would characterize the figure. I found nothing of the sort,

[41] Three samples are given in Fig. 1.

however, and my only reason for giving this word is that it appealed to me a bit more than any other word there" (*Gl* 25).

This example, and many others which we might give, show most clearly that the total naming-process, under our conditions, is far from a simple and logical procedure. If we are to trust the reports, the process does not usually take a straight uninterrupted course, but involves many changes of direction. Self-instructions of the most diverse forms appear to redirect the search for the best name; there is much verbal comment, and imaginal resources of various kinds are drawn into service.

The bases or reasons for choosing the names were reported to be of many different forms, but may be conveniently classified as follows:

(*A*) *Associative bases.* In this type of naming, the *O* reports that he has relied upon his linguistic resources or upon his knowledge of familiar objects or upon both. Various sub-forms may be distinguished.

(1) The nonsense sound-complex may suggest a familiar word which suits the figure or a part or aspect of it.

"The *peg* at the end of the word (*wopeg*) seemed to fit the straight vertical line" (*Er* 3). "It sounds like *jagged* (*jegid*). It was a kind of jagged drawing" (*Lo* 10).

(2) The figure may be given a meaning which suggests an English word; then a sound-complex which resembles this word or a word related in meaning is selected as the name for the figure.

"The figure could be taken as a human face. I looked for something relevant to humans in the words. The *jim* was sufficiently suggestive of a diminutive name, so that I fairly definitely selected the word (*jimex*)" (*Ro* 25). "Because it looked like a door; the nearest thing to a door was a sill; and the nearest thing to sill was *silafol*" (*St* 5).

(3) The sound-complex falls into the context of a particular language or group of languages.

"The word (*quox*) suggested a Latin word, not any particular Latin word, but just something Latin, while the figure might have been a Roman numeral written in a hurry" (*Er* 47). "It seemed to me that it was possible that *zicomez* might be the name which the Indians might give to such a design" (*Gl* 20).

(*B*) *Attributive bases.* In these cases, the word and the figure are apprehended as being alike or even identical in certain perceptual respects. This form of matching is similar to, but broader than, Usnadze's configurative relationship.

(1) In a first sub-form, the matching is in terms of formal properties of figure and word.

"Had something of the desired roundness about it" (*Gl* 33). "The rather sharp vowel-sounds of the word (*niken*) harmonized with the two sharp angles of the figure" (*Gl* 51). "I selected *pelakak* because the syllables seemed to be square and discrete" (*St* 79).

(2) Sometimes the fitting is in terms of other properties which can hardly be called configural or formal.

"The one which I selected (*ropiquov*) had a frivolous sound . . . " (*Gl* 11). "The word (*jicum*) seemed a combination of hard and soft, and the figure gave me that impression too" (*Er* 14).

(3) The *appearance* of the typewritten word may fit the appearance of the figure. This form of matching is a by-product of our method, resting as it does upon visual perception of the word-pattern. Few cases fell within this sub-group.

"The *z*'s looked like straight lines and angles" (*Er* 13).

(*C*) *General impression.* The *O* relies upon a general impression and there is no thorough attempt to rationalize or explain the interconnection.

(1) Such a connection may be in terms of 'feeling.'

"Immediately the first one (*rowuv*) seemed appropriate, because the sound of it seemed to fit the feeling which I got from the circular, round aspect or characteristic of the figure. Then, however, I said to myself, 'I must look at the other words, because some one of them may be more appropriate. None of them would do, however, and so I sat back feeling very pleased at the first choice I had made. As the exposure ended I was engaged in trying to find reasons why that particular word fitted the figure so well, but I found nothing more definite than I have mentioned" (*Gl* 1). "I just had a feeling this was better. I had no reason for it" (*Lo* 43).

Gl sometimes appealed to empathy and kinaesthesis.

"The meaning of either figure or word was not definitely formulated, but the two went together empathically very well. . . . I found it extremely difficult to get any further behind this emphatic belonging-togetherness of figure and word" (*Gl* 54). "There was something about the word . . . which made it seem appropriate in a very vague and ill-defined way. This feeling of the appropriateness of the word to the figure is something I find entirely impossible to put into verbal terms. The nearest I can approach it is to say the sound and kinaesthetic feel of the word which I got when I pronounced it to myself several times tended to harmonize with the feel which I had for the figure. I hesitate to say that this feel for the figure was kinaesthetic, because it wasn't given to me at the time as such. It was vague and diffuse, so that it would be unfair to state that I had it in terms of anything so definitely articulated as kinaesthesis" (*Gl* 5).

(2) Sometimes no 'feelings' are mentioned. It is difficult to distinguish between this and the preceding sub-group, their difference being one of degree of emphasis rather than of kind; sometimes the difference is simply a matter of terminology.

"The word (*mopas*) seemed, for some reason I couldn't understand, to fit the figure" (*Er* 72). "I had accepted it as being an appropriate name, and I now was unable to go beyond that, any more than I can say why *house* is the name for the object we so designate" (*Gl* 34). "As soon as I saw the word, I was immediately prejudiced in its favor" (*Lo* 41). "I returned to *rohom* and decided upon it. I don't know why . . . I do not understand the connection" (*St* 96).

(D) Negative cases. This group seems necessary for the disposal of cases in which a word is reported, in accordance with the instructions, but the *O* can discover no real reasons for making that choice and furthermore is not at all satisfied with the word.

"I felt that I had selected a word which was utterly incongruous to the figure" (*Gl* 13). "I chose that word merely because I happened to be looking at it" (*Lo* 77). "I selected *raral* because it seemed as good as any" (Ro 54).

(E) Partial bases. Here the *O* is partly, but not entirely, satisfied with his choice. Cases in this group fall between those in Group D on the one hand and those in Groups A-C on the other.

"There was something about it which fitted the figure slightly. . . . I feel unsatisfied with my choice" (*Gl* 3). "It (*pukapik*) is good for describing a criss-cross of forces; but it's not quite forceful enough" (*Lo* 78).

In Table IV we have listed the number of reports falling under each heading and also the number of each type of report given by each of the *O*s. As the table shows, the *O*s differed a great deal in the frequency of various types of report.

If, in accordance with Usnadze, we combine the 'general impressions' and 'attributive' selections into an 'intrinsic' or 'objective' group, it is striking that *Lo* and *Gl* gave many more reports of such a type than did *Ro* and *St*, and that *Ro* and *St* gave many more associative reports than did *Gl* and *Lo*. *Er* fell between the two groups of *O*s in both respects. These extreme differences are probably due in large measure to the fact that *Gl* and *Lo* had, just before our experiments, been *O*s for Bentley and Varon. As a result of their participation in this earlier experiment, *Gl* and *Lo* were undoubtedly working under an *Einstellung* favoring 'general impression,' or, more particularly, 'attributive' criteria.

Certain incidental observations, especially by *Gl* and *Lo*, whose reports tended to be fuller than those of the other *O*s, are of great interest. *Gl* commented very frequently that the reasons which he had given in his reports were results of rationalization.

"Then I found I didn't know why that particular word fitted it so well. I set about trying to find reasons for my choice" (*Gl* 5). "I could make up reasons, but I really had no reasons" (*Lo* 11). "I went back and tried to build up a case for *pelakak*. It was very easy" (*Lo* 79).

Nothing in the reports given by the other *O*s contradicts the *possibility* that many of the reported reasons are reasons discovered or elaborated *after* the choice of the name. *Gl* was, almost from the beginning, fully cognizant of the distinction between real reasons and others, but was very often unable to make clear distinctions between them in particular reports. This general tendency toward rationalization, toward enrichment of a possibly already existing interconnection between figure and name, has, as we shall see, important implications.

TABLE III

AGREEMENTS AMONG *O*S IN SERIES B

Pattern of agreement	A priori probability	Expected frequency	Empirical frequency
A^5	0.00077	0.08	0
A^4B	0.01929	1.93	9
A^3B^2	0.03858	3.86	6
A^3BC	0.15432	15.43	19
A^2B^2C	0.23148	23.15	26
A^2BCD	0.46296	46.30	34
ABCDE	0.09259	9.26	6

In Table III are listed the various possible types of agreement between the *O*s, the a priori probabilities of their occurrence, the expected frequency of each in 100 trials, and the actual empirical frequencies found in this experiment. In this particular series, as well as in Series A, there was an obvious source of error which makes it impossible for us to draw the tempting inference suggested by the table and which would make a further statistical analysis of these results misleading. We found, after the series had been concluded, that the *O*s had tended to select words occupying certain favored spatial positions on the cards. Each *O* selected, on the average, 22.6 words in the first (top) position, 17.2 in the second, 19.8 in the third, 14.0 in the fourth, 10.8 in the fifth, and 15.6 in the sixth (lowest) position. A 'space-error' thus presumably played a considerable part in determining the agreements in this series; we were able to show in later series, however, that this spatial factor is neither the essential nor the sole factor

underlying agreements such as those we have found here. The 'space-error' is interesting in another connection, in that it shows how a purely irrelevant criterion, such as spatial position, may be a real determinant of selection, especially if, as here, as many as five or six words are presented with each figure for a short period of time.

We have considered independently the verbal reports of the *O*s and the extent of agreement between them in their choices of names. It is now pertinent to ask to what extent there is agreement in the *contents* of the reports given by several *O*s in cases where they have selected the same name. We shall consider here only the 34 cases in which 3 or 4 *O*s agreed upon one and the same name. Among the 9 cases of agreement by 4 *O*s, there was not a single case in which either 4 or even 3 of the *O*s agreed in the 'reasons' for their choices. In three cases, 2 *O*s agreed upon reasons, *e.g., St* and *Er* both reported associations of *peacock* with *pegok* (no. 26). Turning to the 25 cases of agreement upon the same name by 3 *O*s, we note that in one, and only one, case did all 3 *O*s agree upon a 'reason'; *Gl, Er,* and *Lo* reported *zitix* to be a good name for 'crosses' (no. 69). In six of these 25 cases, 2 of the 3 *O*s agreed more or less upon their reasons.

An example of the sort of disagreement typical of the other cases follows: *St* reported an association of *jonquil* with *dajiquil; Lo* was "immediately prejudiced" in favor of the word; *Ro* reported that the word was similar to the name of an Indian tribe and that the figure suggested arrows; *Er* found the word "as good as any" (no. 41).

Of the 10 positive agreements in 'reasons,' 4 are clearly agreements upon an associative criterion and one may be considered as possibly associative. Only the remaining cases can be considered as being in any sense agreements upon intrinsic bases; and of these probably not more than 2 or 3 are entirely convincing. These results are decidedly contrary to Usnadze's finding that agreements upon a name were produced in all except one case by intrinsic factors.

There is, of course, a logical possibility that choices of the same name by different *O*s are really determined by different factors, but the nature of our material and method was such that this possibility may be disregarded. We are consequently left with the implication that the *real bases* for selection of names are not, in general, revealed in the *O*s' reports. We cannot with any high degree of reliabilty determine from one report the real 'cause' for the selection of the name.

Series C

Series C was undertaken to determine to what extent the *O*s tend to select the same names as before when material which has been used pre-

viously is reëxposed. It was performed about 2 mo. after Series A, Series B having been completed during this interval.

Procedure. The *O*s were the same as those in Series A; and the cards used for reëxposure were those which had been used in Series A, but they were now presented in different order. The instructions were:

"After the signal 'Ready—now,' I shall expose for ten seconds a figure and six nonsense words which you have already seen together. Find the word which you most probably selected before. After the exposure, report the word, a number indicating how sure you are that you have succeeded in refinding the word you gave before, and any comments you can make about the experience."

The *O* was given a card upon which were indicated the numbers to be used for various degrees of certainty: 1 = absolute certainty; 2 = high certainty; 3 = moderate certainty; 4 = slight certainty; 5 = complete uncertainty (nothing more than a guess).

Results. Since this series was very brief (70 observations) we shall not undertake a detailed analysis of the results. The average number of correct reproductions (*i.e.* reproductions of words already chosen in Series A) was 9.4 out of the 14 for each *O*. Words which had been selected in the first series upon associative bases were given again in this series in 70.8% of the 24 cases; those selected upon attributive bases, in 75.0% of the 4 cases; and those selected by irrelevant criteria or those reported in Series A to be inadequate (negative cases) in 54.5% of the 22 cases. Usnadze found that words which had been selected on the basis of intrinsic criteria were chosen again in the second series "fast in allen Fällen" and that in cases falling in the associative group the *O* was in the second series "sehr haufig irregeführt." Our data offer no marked confirmation of Usnadze's statements.

From the degrees of certainty reported by the *O*s we can draw certain generalizations, if we make the assumption that the 5 degrees of certainty are equidistant for statistical purposes. The mean certainty of the *O*s in cases in which the correct word was reproduced was 2.94 (certainty somewhat better than moderate); in cases in which the correct word was not given in Series C, 4.11 (very slight certainty). This calls for no comment. If we classify all the cases in 8 groups in terms of (a) correctness or incorrectness of reproduction in Series C, and (b) basis reported in Series A, we find these average certainties: correct associative, 2.18; incorrect associative, 4.29; correct attributive, 3.73; incorrect attributive, 4.00; correct general impression, 3.33; incorrect general impression, 4.00; correct negative, 3.58; and incorrect negative, 4.10. The most outstanding difference among the correct groups is that between the average certainties of correct reproduction of words selected upon associative and of those selected upon attributive bases, the difference being 1.55 units upon the scale of certainties. This difference is changed only very slightly if we combine with the attributive cases those of general impression. This difference might be considered an indirect confirmation of Usnadze's statement that his *O*s typically did not "know" in the second series what word they had chosen in the first series when the basis had been intrinsic. In our 17 correct associative cases, there were 5 reports of absolute certainty and 8 of high certainty; in the 18 correct intrinsic cases, there were no reports of absolute certainty and only two of high certainty.[43]

[43] But see below, pp. 565-66.

Series D

Series D bears the same relationship to Series B as did Series C to Series A.

Procedure. The procedure was in general the same as in Series C, except that of course we now used the 100 cards which had been previously used in Series B. The order of the cards within each group of 10 was changed. The series began about 2 weeks after the completion of Series B and about 2 mo. after the beginning of Series B.

Results. Since our results in this series are much more numerous (500 observations) than in Series C, we may undertake a more detailed analysis. The *O*s' reports indicate that the selection of the word most probably selected in Series B may depend upon several quite different criteria which sometimes are involved singly and more frequently coöperate in various ways.

(1) O reported that he made his choice in this series simply because the word selected was more familiar than any of the others.

"I choose that word because it was the only one which seemed to be familiar" (*Er* 9+).[43]

In a surprisingly large number of reports, the *O*s mentioned that a particular word seemed to "stand out."

(2) O reported that he recalled the basis for his original choice and thus was able to choose now what seemed to be the correct word.

"I remembered my interpretation of the figure before looking at the words. I read the words over twice before actually realizing that *meg* was there. When I did stop to think about this word, I recalled having chosen it and why" (*St* 7+).

(3) O found the material on the card unfamiliar and was unable to recall his choice; but now, in looking at the card, he found a basis for choosing some particular word, and this in turn made it seem probable that the same word had been chosen in Series B. In many, but not all, of such cases, the *O* made a 'correct' choice, *i.e.,* his original word.

"I don't know whether it was the word I chose before or not, but it was certainly the one I chose now as best fitting the picture" (*Lo* 7+).

In fewer cases, the reproduction was incorrect.

(4) O reported that his choice was simply a guess.

"I have no reason for choosing it" (*Lo* 27—).

In many cases the *O* reported a combination of the criteria of familiarity and suitability of the word.

The average number of correct reproductions for each *O* was 56.4 out of the 100. At the end of the series each *O* was asked to guess the number of correct reproductions he had made. These numbers were in every case less than the actual number of correct reproductions, their average (40.0) being 16.4 less than 56.4.

Making the assumption that the 5 degrees of certainty may be considered equidistant for statistical purposes, we found that with every *O* the mean certainty for the correct reproductions was reliably higher than that for the incorrect reproduc-

[43] The symbol '+' refers to cases in which *O*'s choice agrees with his choice in Series B; the symbol '—' indicates lack of such agreement.

TABLE IV

QUANTITATIVE DATA OF SERIES D

	Os					
	Er	*Gl*	*Lo*	*Ro*	*St*	All
No. correct	53	68	48	51	62	282
No. incorrect	47	32	52	49	38	218
Av. certainty, cor. repro.	3.226	2.956	2.917	3.490	3.565	
S. D.	0.92	1.10	1.34	1.11	1.45	
Av. certainty, incor. repro.	3.957	4.375	4.308	4.449	4.790	
S. D.	0.85	0.74	0.89	0.61	0.52	
Diff. between av. cor. and incor.	0.731	1.419	1.391	0.959	1.225	
S. E. diff.	0.18	0.16	0.23	0.18	0.20	
Diff. sigma-units	4.1	8.9	6.0	5.3	6.1	
Reliability of diff.	0.999	0.999	0.999	0.999	0.999	
Associative criteria:						
No. cases	46	21	6	71	77	221
No. correct	21	17	3	40	49	130
Av. certainty, cor. repro.	2.95	2.47	3.33	3.27	3.29	3.13
Attributive criteria:						
No. cases	31	24	37	—	5	97
No. correct	20	18	21	—	3	62
Av. certainty, cor. repro.	3.41	2.89	3.05	—	4.00	3.17
General impression:						
No. cases	10	24	13	5	10	62
No. correct	5	15	6	1	3	30
Av. certainty, cor. repro.	3.60	2.60	3.33	3.00	4.33	3.10
Partial bases:						
No. cases	—	22	13	—	—	35
No. correct	—	12	7	—	—	19
Av. certainty, cor. repro.	—	3.83	3.00	—	—	3.53
Negative cases:						
No. cases	13	9	31	24	8	85
No. correct	7	6	11	10	7	41
Av. certainty, cor. repro.	3.71	4.33	3.00	4.40	4.86	3.98

tions.[44] We should expect this result, and we mention it only because it indicates that this statistical method of dealing with the degrees of certainty is accurate enough to reveal such a significant difference.

We now studied the relationship between the type of report given by *O* in Series B and accuracy of reproduction in Series D. As Table IV indicates, the words originally 'selected' by associative criteria, attributive criteria and general impression—and reproduced correctly in Series D—were reproduced with approximately the same average degree of certainty, the averages being 3.13, 3.17 and 3.10 respectively. The words which did not fit the figure well (partial and negative groups) and which were reproduced correctly in Series D were reproduced with lower degrees of certainty, 3.53 and 3.98 respectively. In this series there appears

[44] See Table IV.

to be no agreement with Usnadze's implication that names selected by intrinsic criteria are reproduced with less certainty than those selected by associative criteria.

There also appears to be no significant difference between the average accuracies of reproduction of words selected upon associative, attributive and general unrationalized bases. Of the words selected by associative criteria in Series B, 58.8% were reproduced correctly in this series. The corresponding percentages for words chosen by attributive criteria and by general impression are 63.9% and 48.4%, and for these last two groups taken together (Usnadze's intrinsic criteria), 57.9%, which is almost identical with the value for associative criteria (58.8%). Under our conditions, then, the reason given by the *O* for his initial choice has little or nothing to do with the probability that he will later reproduce it correctly; this is at variance with still another generalization made by Usnadze.

Series E

In Series E, we undertook a study of factors involved in inventing, without any essential restrictions, names for nonsense figures.

Procedure. We drew upon separate cards 50 nonsense figures, each consisting of 5 strokes, curved or straight or both. The figures were divided arbitrarily into 5 groups of 10 each, a different group being used with each of the 55 *O*s. The instructions were:

"I shall show you a nonsense-figure. Find a nonsense-word which would make a satisfactory name for it or would fit the figure well. Also construct 2 other words which would be bad names for the figure or would not fit it well. In constructing these words try to avoid any associations at all with English words or words in any language you know. That is, consider the words which you construct merely as sound-complexes. Write the 3 words (the good name first) upon the small card. Then give briefly any comments you can about how you were led to give these words. There is no time-limit, but try to work rapidly."

If *O* gave any words having obvious relationship to any familiar words in the English language, he was asked to substitute other words; this situation arose only a very few times. Also, if *O* selected as a 'good' name a word which was in length radically different from the two 'bad' names, he was asked to substitute for the latter two words standing out less from the 'good' name. This precaution was necessary since we intended later to use the figures and words with other *O*s and wished to rule out, as far as possible, any secondary cues which might aid these *O*s in finding the inventor's 'good' word.

Results. The *O*s were able to act in accordance with the instructions; this is in itself noteworthy, since the operation exacted of the *O*s was unusual. In Fig. 2, we give an example of a figure and the appertaining words constructed by one *O;* the underlined word is the 'good' name. The *O*s found it much easier, in general, to invent good names than bad names.

"I find it harder to make up words which don't fit the figure than the ones which do" (*Er* 9).

In spite of the instructions, 3 *O*s (*Er, Ro,* and *St*) reported the use of associative criteria in a few cases. In most instances, however, the *O*s reported non-associative factors, usually of the sort we have called attributive.

"I immediately got a feeling of movement and my purpose was to analyze the movement into some form that would be described in sound. I decided on the vowels first and then went through the alphabet to find a suitable consonant to begin the

word. I discarded several because they were not sharp enough" (*Lo* 4). "I took as unanalytic attitude as I could to obtain as far as possible a vague total impression, if possible with affective coloring. The figure looked cramped or tortured; it looked repressed, harsh. I selected a word that was harsh-sounding—*gax*. The word didn't come at once but I knew vaguely that I wanted consonants with a hard sound" (*Ro* 3). "The three circles seemed to necessitate a word in which the idea of similar things was brought out, but because of the angle in the figure, the word had to be more than a succession of similar syllables; that was principally the reason the last syllable of the word (*goguzit*) was added" (*Er* 5).

Several reports indicate ambiguity in the figures.

"I found it difficult to find an appropriate sound-combination for this figure, because there were two meanings which I got from it which I could not fit together very well. One meaning was a geometrical one which applied mostly to the arrangement of four straight lines. The other meaning applied to the curved line and was less definite. It was a rather vague empathic or possibly kinaesthetic meaning. Even though this meaning was vague, it was more compelling than the geometrical meaning. I spent some time trying to find a word which would include both of these meanings. But I finally decided it would be necessary to regard the curved line as the dominant part of the figure and to let the rest of the figure serve only as an appendage" (*Gl* 8).

Such ambiguity, which we also found in earlier series, provides one reason for the lack of perfect correlation between figure and name.

SERIES F

Our aim in this series was to study the processes involved in constructing nonsense-figures matching or well named by certain assigned nonsense sound-complexes.

Procedure. We prepared for each of the 5 *O*s 10 lists of 3 nonsense words, constructed according to principles we have already described. The O was instructed as follows:

"I shall show you three nonsense-words, to be pronounced in accordance with the instructions on pronunciation already given. Construct a simple figure for which one of the 3 words presented would be a good or fitting name and for which the other 2 words would be bad, unsuitable names. Try to work rapidly. Avoid associations with words in any language you know, and do not make your drawings reproductions of familiar objects. Consider the words simply as sound-complexes and your drawing simply as a visual pattern."

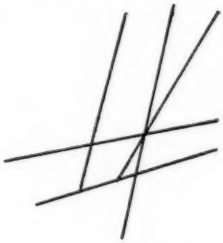
Results. Samples of the words and drawings are shown in Figs. 1 and 2. The underlined word in each group is the word used by the *O* as the 'good' name for the figure. Three of the *O*s commented that the task in this series was much easier than that in the preceding series.

"It's much easier to draw pictures for the words than to get words for the pictures. . . . In giving a word one feels that one is committing oneself to something definite, whereas the pictures can be interpreted in so many different ways" (*Lo*). "I had little difficulty; the figure seemed to suggest itself, either immediately or after thinking it over for a short time" (*Er*).

Associative criteria were reported only rarely in this series, the *O* typically reporting intrinsic bases.

"This (*lis*) was definitely slow and soft. It seemed that this one should be done entirely in curves. It was definitely a soft figure" (*Ro* 12).[45] "It (*sisifit*) suddenly

[45] Numbers from 11 to 20 are used to designate the figures and words of Series F; numbers from 1 to 10 were used to designate those of Series E. Also, '*Ro* 12' and '*St* 12,' for example, would refer to quite different figures and words.



murari
petiva
jixika

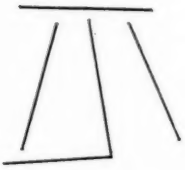
Series E Gl 7

murari
petiva
jixika

Series H Gl 7

lep
malal
puquit

Series F Lo 19



lep
malal
puquit

Series H Lo 19

quilat
manuv
luribub

Series F Ro 16

quilat
manuv
luribub

Series H Ro 16

pasitel
bohum
minigib

Series F St 20

pasitel
bohum
minigib

Series H St 20

FIG. 2. EXAMPLES OF WORDS AND FIGURES

seemed to me to be a very zigzag word with a drop at the end" (*Lo* 11). "*Malal* has a kind of waving motion" (*Lo* 19).

The most difficult aspect of the problem was for *O* to construct a figure which as a total was well named by the word as a total. All the *O*s commented, from time to time, upon the need for revising certain figures after they had drawn them in order to do justice to certain aspects of the word as a whole which had been neglected.

"The center part represents the sounds of the separate syllables, whereas the outer part was put around it because it (*mebipit*) seemed to have a certain unity" (*Ro* 18). "It looked too diagrammatic, too bare, to suit the word (*piz*) which does sound a bit frivolous, so I added the two short lines at the side" (*St* 13).

Series G

We undertook in Series G a study of the agreements between the inventors' choices of names and figures in Series E and F and the choices made by *O*s who had not previously seen these cards.

Procedure. We utilized the figures and words of these two earlier series. The material thus consisted of 100 figures, each combined with three words. We drew the figures and typewrote the words on 5 x 8 in. cards, and exposed them by means of the apparatus used in earlier series. Since each *O* had already named 10 figures and drawn figures for 10 words, there were left 80 cards which he had not yet seen. This present series consisted, accordingly, of 80 observations by each of the 5 *O*s, although in our statistical treatment of the results we have also taken the data of Series E and F into account. The instructions were:

"I shall expose a visual figure and 3 nonsense-words for 6 sec.[46] Find that one word among the 3 which best fits the figure or is the best name for it. Consider the figure as simply a nonsense-pattern and the words as simply nonsense-combinations. After the end of the exposure, report first the word you have chosen, and then give a brief concise commentary about the experience."

Results. In accordance with the change of instructions, the attitudes of the *O*s, as reflected in their comments, were generally different from those in Series A and B. There were, in the 400 observations constituting Series G, only 24 reports of associative criteria, while there were 173 reports of attributive criteria and 89 of general impression. The remaining 114 reports, which we may place in a 'doubtful' group, we have not attempted to classify; in many of such cases, *O* reported that the word chosen was not strictly a name for the subject, and in others it was impossible to determine from the report the nature of the criterion involved. *Er's* shift of attitude is characterized in the following general comment:

"In the very first series (Series A and B), in order to have a word fit a figure I had the idea that they must somehow express the same idea, that of some particular object or something like that. However, now certain syllables and sounds represent certain curves and angles and some combinations express something fairly compact and some of them sort of drawn out" (*Er*).

Some of the typical characterizations of words in this series are as follows:

Luribub: rambling, indeterminate; *lixifiz:* straight, long, complex; *muk:* concise, small; *pexizi:* concise, angular; *bul:* curved; *mixik:* hard, angular (by *Er*).

[46] After the first few observations, the exposition-time was lengthened to 8 sec. for *Ro*, since he commented that a period of 6 sec. was too short for him.

Minip: lively; *zapin:* voluminous, yet pinched; *vip:* small, concentrated, sharp; *mabir:* curved, dynamic, lively (by *Gl*).
Zomil: curved (by *Lo*).
Labax: hard; *shrok:* crisp, sharp; *texali:* not well integrated (by *Ro*).
Blog: heavy, square, solid; *bul:* heavy (*b*), smooth and soft (*ul*) (by *St*).

In general, there is very imperfect agreement between the *O*s as regards the 'symbolism' of particular sounds, and even for one and the same *O*, there is not always a perfect consistency in the characterizations reported. *St* was different from the other *O*s in this respect; she alone developed a 'system' of characterizing the sounds. At the end of the series, *St* was asked to describe the 'symbolic value' of the letters of the alphabet, as pronounced in accordance with our instructions. A few examples of her comments about the speech-sounds are as follows:

a = "something extended."
b = "weight or bigness which meant weight."
d = "some heaviness or height, without too much thinness; something that gives height with heaviness at the bottom would be excellent. That is rather more visual that auditory."
l = "a long, slightly curving line, or even part of a circle."
m = "an arc or a curly line."
q = "a short, thin straight line, or an angle."
s = "a curlicue, probably visual. *It seems a bit auditory, but I imagine that is rationalization.*"

This elaborate system reported, and fairly consistently utilized by *St*, indicates very clearly the wealth of characteristics which speech-sounds may have. In a few cases the 'symbolism' accrues to the appearance of the written letter rather than to its sound, but in general *St* is inclined to attribute characteristics to the sounds themselves. The sentence which we have italicized above shows that *St* was quite aware of the danger of rationalization.

We found in this series (as in others) that the *O*s agree very little in the reasons which they give for choosing the same words. In 15 of the 100 cases all 5 *O*s agreed upon one and the same word as the best name for the figure. The only agreements in reported bases which can be found among these are as follows: 3 *O*s found angularity or hardness or sharpness in *hexit;* 2 *O*s called *xex* tangled or confused; 2 referred to rotundity or curvedness in characterizing *bul;* 2 called *rakok* harsh or angular or sharp; two spoke of an emphatic harmony between *gajan* and the appertaining figure; 2 reported associations of *tirogon* with the word *polygon;* and for 2 *O*s *rililin* suggested a series of similar arcs. Otherwise the *O*s disagree concerning their bases for these 15 names or simply do not report reasons. As in other series, we find a correlation far from perfect between word selected and 'reason' for its selection.

In one, and only one, way have we been able to make out systematically on the basis of the *O*s' reports an 'objective' relationship between sounds and their characterizations. An inspection of all the reports of attributive bases revealed that *sharpness* and *angularity* on the one hand, and *roundness, smoothness* and *voluminousness* on the other were the terms most often applied to figures and sound-complexes. Logical considerations, supported by some of the reports, permit us to treat these two descriptive groups as opposites. We determined the number of times each letter was characterized as *round, smooth,* etc., and as *sharp, angular,* etc. If an *O* characterized an entire word in either of these ways, we took into account all the letters in the word; if the *O* specified that only certain particular letters in

the word were so characterized, we counted only these letters. In Table V we give the number of times each letter was characterized in each of the two ways and, in terms of percentage, the relative number of times it was reported as 'sharp,' etc.; these percentages subtracted from 100% would indicate the relative frequencies for 'roundness,' etc. As the table immediately reveals, some of the letters were characterized in the two ways so infrequently that safe inferences about their 'symbolic' value cannot be made; the letter *d, e.g.* was characterized in the two ways only 9 times. There are, on the other hand, many letters which were very often so characterized. If we disregard letters with total frequencies of less than 20, we are left with this rank-order, running from greatest sharpness or angularity to greatest

TABLE V

RANK-ORDER OF LETTERS IN SHARPNESS AND ROUNDNESS

Letter	No. times characterized as "sharp," etc.	No. times characterized as "round," etc.	Relative frequency of characterization as "sharp," etc.
d	8	1	88.9%
j	13	2	86.7%
x	15	3	83.3%
v	12	3	80.0%
i	65	19	77.4%
z	16	5	76.2%
k	22	7	75.9%
w	3	1	75.0%
f	8	3	72.7%
q(u)	5	2	71.4%
h	4	2	66.7%
p	12	7	63.2%
g	13	10	56.5%
s	9	7	56.3%
t	11	9	55.0%
a	18	27	40.0%
e	13	20	39.4%
c	3	5	37.5%
y	3	5	37.5%
o	10	29	25.6%
n	4	13	23.5%
r	6	20	23.1%
b	5	23	17.9%
u	8	39	17.0%
l	2	42	4.5%
m	1	32	3.0%

roundness, smoothness or voluminousness: *i, z, k, g, t, a, e, o, r, b, u, l, m*. The fact that we have been able to discover such a scale, imperfect as it may be, is significant for two reasons. First, the *O*s were not given any formal instruction to search for these particular 'symbolisms.'[47] Secondly, we have been able to find great differences between the two extremes of the series in spite of the fact that we counted *all* the letters in many of the characterized words. Although undoubtedly many of the particular letters in these words did not fall in with the 'symbolism' reported for

[47] Since both angles and smooth curves were utilized a great deal in the construction of our figures, the performances of the *O*s was probably governed to some extent by a type of instruction arising from the situation itself. Cf. M. Bentley, *The New Field of Psychology*, 1934, 29, 366.

the entire words, there still remains a great difference between the extremes. The scales established by Newman have much greater reliability than ours, but it will be recalled that he was working particularly with contrasts between pairs of sound-complexes whose members differed in *only one* phonetic element.

If causes subsumed under the category of chance were the only effective factors producing agreements between *O*s, we should expect that each *O* in Series G should agree with the inventors (of Series E and F) upon one-third of the names, *i.e.* upon 6.67 names, since there were 3 words upon each card. We find, however, that the *O*s in Series G agreed, on the average, with 9.50 of the 20 words designated as best by the inventors, and in no case were there fewer than 7 agreements.

In Table VI we present the number of cases in which various numbers of *O*s in this series agreed with the inventor. We have added, in the table, the probabilities of occurrence of each of the types of agreement, and the expected number of cases of each. It is immediately noticeable that there is a very marked tendency for the *O*s to agree with the inventor. Through application of the Gram-Charlier series[48] we determined the probability of the occurrence of as many as 15 cases (out of 100) in

TABLE VI

AGREEMENTS BETWEEN INVENTORS AND OTHER *O*S IN SERIES G

No. *O*s agreeing with inventor	A priori probability	Expected frequency	Empirical frequency
4	0.0124	1.24	15
3	0.0988	9.88	23
2	0.2963	29.63	18
1	0.3951	39.51	25
0	0.1975	19.75	19

which 4 *O*s agree with the inventor, and also of the occurrence of as many as 23 cases (out of the 100) in which 3 *O*s agreed with the inventor. In each case the probability was so low (less than 0.0001) that the agreements can hardly be attributed to chance. Since the differences indicated in the table are so striking, it seems unnecessary to give a detailed report of our statistical procedure.

Spatial positions of the words upon the cards did not contribute to any significant extent to the agreement of the *O*s. Of the 400 names chosen, 127 were in the first position, 127 in the second, and 146 in the third position on the card. There was, in this series as in others, a greater tendency for the *O*s to choose polysyllabic than monosyllabic words, on the average each *O* selecting 25.1% of the available monosyllabic, 34.5% of the disyllabic, and 40.0% of the trisyllabic words. The results of Series H show, however, that absolute characteristics of the words cannot be the essential determinants of agreements, and also confirm our supposition that, under the conditions of this experiment, the space-error is negligible.

The *O* reported voluntarily in most of the 400 observations constituting this series that the name selected was either 'good' or 'bad' (even though better than the other possibilities). Reports which were at all doubtful we divided equally among the two groups. We may also classify all the reports by another criterion—whether or not the word selected by the *O* in this series is the same as the name

[48] Cf. B. H. Camp, *The Mathematical Part of Elementary Statistics*, 1931, 225 ff.; A. Fisher, *The Mathematical Theory of Probabilities*, 1, 1926, 188 ff.

assigned to the figure by the inventor in Series E or F. Combining the criteria of agreement with inventor and goodness of fit, we may classify all of the 400 reports of the present series in four groups. Of the words which agreed with the inventor's selections, 143 were reported to be good names, 47 to be bad or questionable names. Of the words which did not agree with the inventor's selections, 114 were good names and 86 bad. These results suggest a positive relationship between goodness of fit and agreement with inventor. If the *O* selects an 'incorrect' word, he is about equally likely to find it good or bad. If, on the other hand, he selects a word already assigned to the figure by the inventor, he is very much more likely to call it 'good' than 'bad,' even though he will probably not specify the same reason for selecting it as did the inventor.

Series H

Series H was included in order for us to determine whether or not the agreements between the *O*s' selections of names in earlier series can be attributed primarily to certain absolute characteristics of the sound-complexes themselves, taken in isolation from the figures, or whether the agreements involve real relationships between figures and words.

Procedure. In Series G there were 41 cases in which 4 or 5 *O*s selected the same name. In Series H, we retained the 41 sets of 3 nonsense sound-complexes, but instead of using the figures of Series G, we drew new figures, each of which differed as much as possible from the one upon the corresponding card in the earlier series. Round figures were replaced by angular; large by small; relatively complex by relatively simple; symmetrical by asymmetrical; etc. It was usually difficult, if not impossible, to draw a figure which might be regarded as the 'opposite' of the corresponding figure of the earlier series; in many cases, we could do nothing better than produce a figure which was simply unrelated to the earlier one.

The figure and words were exposed as in earlier experiments, and the instructions were essentially the same as before. The *O*s were *Er, Lo, Ro,* and *St,* all of whom had observed in Series G,[49] and also Dr. R. B. MacLeod (*Ma*) instructor in psychology, and Mr. A. D. Freiberg (*Fr*), graduate student in the department.

The main problem of the series was to determine whether *O*s still tended to pick the same words after the figures had been changed. If such is the case, we must assume that certain absolute characteristics of the words underlie the agreements in such series as Series G. If, on the other hand, the *O*s tend to shift to other words, we should have an indication that the choice made by the *O* depends upon relationship of some sort between figure and word.

Each of the 4 *O*s who had taken part in Series G chose on the average in Series H 13.0 of the 41 words which he himself had chosen in Series G; this is slightly below what we should expect by chance (13.67). We also find that each of the 6 *O*s chose on the average in this series 12.7 of the 41 words which had been selected by either 4 or 5 *O*s in Series G, the expected chance frequency again being 13.67. There is therefore no tendency, so far as can be discovered, for the same words to be named in the two series.

[49] *Gl,* the remaining *O* of Series G, was unavailable.

We determined the number of times each of the 6 *O*s in this series agreed in his selections with each of the other 5 *O*s in this same series. The expected number of agreements would be, as before, 41/3 or 13.67. Every one of the empirical values is larger than this, the smallest being 15, the largest 25. The average number of agreements between pairs of *O*s in this series is 19.3.

Absolute characteristics of the words are evidently insufficient to explain agreements. When figures are changed, names tend to change. It may appear strange that in spite of the change of figures, there is still approximately chance agreement between choices made in Series G and H, for with opposite or contrasting figures we might expect, ideally, *no* agreements. The solution of this difficulty is undoubtedly that between the pairs of figures of the two series there is not so much a relationship of opposition or contrast as a simple lack of relationship; under such conditions we should expect chance agreement between the two series.

Series I

In the earlier series we employed 6 *O*s at the most. In Series I we undertook to study agreements in names selected and reasons reported by a large number of *O*s.

Procedure. We utilized as material simply the two figures and two words—*takete* and *baluma*—given by Köhler[50] and shown in Fig. 1, a and b. The *O*s, numbering 78, were members of four classes in elementary psychology. Different procedures were employed with the four groups. In Group I we reproduced upon the blackboard Köhler's two figures and two words. The *O*s were instructed to match the figures and words and to write any relevant comments. *E* pronounced the words aloud, accenting the second syllable of each and giving the vowels the same pronunciations as in the other series. In Group II, Fig. 1a and the two words were reproduced; the *O*s were instructed to select the word better naming the figure and to give their comments. Later Fig. 1b was added and the instructions used in Group I were given. In Group III the procedure was the same, except that Fig. 1b was first used, Fig. 1a being added later. In Group IV, both figures were drawn but at first only the word *takete* was presented, the *O*s being instructed to select the figure better named by this word; later we adopted in this group the procedure used in Group I. The other possible variation in the procedure appeared to be needless.

Results. The results were practically the same under all the sets of conditions. All but 4 *O*s named Fig. 1a *baluma* and Fig. 1b *takete,* and 3 of these dissenting choices were made only when one of the words or one of the figures had not been presented. Many *O*s reported associations of *baluma* with such English words as *balloon* and *ball* as their bases for selection,[51] and a very few *O*s reported associations of *takete* with such words as *kite.* Typically, the *O*s reported attributive similarities or identities between figure and sound-complex.

"*Baluma* gives me the sensation of plumpness, a sort of round roly-poly sensation, and therefore I place it with Fig. 1a." "*Takete* seems sharper and therefore would

[50] W. Köhler, *Gestalt Psychology,* 1929, 242-243.

[51] We made supplementary observations in a class of 37 undergraduates, using *malumba* instead of *baluma.* All but one of these *O*s made the expected matches. This indicates that associations with *ball, balloon,* etc., are not essential in producing agreements.

apply to a figure having points." "The figure is jagged and *takete* sounds that way."

If we disregard the associative criteria, we find startling agreement among the *O*s even in the words which they used to characterize the word and figure in each pair. The word *round* (including its grammatical variants) was used by no less than 26 *O*s to describe baluma and Fig. 1a; smooth and soft were used 16 and 14 times respectively. With the other word-figure pair, *sharp* was used as a descriptive term by 37 *O*s (nearly half of all the *O*s), angular by 9, and even *staccato* by 6.[82]

It seems unquestionable that Köhler selected an extreme example to illustrate his point. The fact that the two words and likewise the two figures are strongly contrastive would tend to exaggerate any tendency toward matching that there might be, an outstanding gradient of sharpness (or roundness) being established. In the case of *baluma*, Von Hornbostel's characterization of the sound *m* seems to be applicable.[83] Furthermore both *takete* and *baluma* are so constructed that they fall nicely into the opposed phonetic classes distinguished by Westermann.[84] The results are also in very close agreement with our own results in Series G, in which we found *m, l, u,* and *b,* to be the roundest or least sharp speech-sounds; these are precisely the important phonetic elements in *baluma* (when the second syllable is accented). We likewise had found that the outstanding sounds in *takete* were sharper (or less round) than the sounds in the other word, even though these sounds were not at the top of the scale in sharpness.

Series J

About 13 months after the completion of Series G we were fortunate enough to find available 3 (*Gl, Ro, St*) of the 5 *O*s who had participated in that series. Since presumably these *O*s had 'forgotten' the words and figures of that series after such a long interval, it seemed of interest to determine to what extent these *O*s would agree or disagree with their own earlier selections of names in Series G.

Procedure. The procedure was essentially the same as in the earlier series. We presented the same cards, in different order; the *O*s were not informed that an earlier series was being repeated.

Results. To dispose immediately of a possible objection to this series, we should say that with a very few exceptions the words and figures appeared quite unfamiliar to the *O*s. *Gl* reported after the completion of the series that he had recalled "three or four" of the 100 words; *Ro* found a few of the "crudely drawn figures" (those of Series F) somewhat familiar, but remembered none of the words and none of the "previous connections" between figures and words.

The expected number of agreements between the choices made by any one *O* in Series G and his choices in this series would be 1/3 of 100, or 33.33, if chance were the only determinant of agreement. *Gl,* however, agreed in 60 of the 100 cases with his original selections, *Ro* in 35 cases, and *St* in 54 cases. The mean number of agreements was thus 49.7. If we neglect the very slight possibility of positive 'recall' by the *O*s of their selections made many months before, we may

[82] The relationship between *takete* and *staccato* is probably associative in part. But, over and above this, the sound of *staccato* is onomatopoetic.

[83] Cf. above, p. 547.

[84] Cf. Table I.

conclude that there are objective factors determining these selections of names objective in the sense that there is a marked tendency for the same *O* to select the same word at different times as the best name for a given figure. We also found that the words selected by the same *O* in *both* series tended to be those which had been selected in Series G by relatively many *O*s. On the average, 3.13 of the 5 *O*s in Series G had selected the words reproduced 'correctly' by *Gl* in this series, and only 2.73 *O*s those reproduced 'incorrectly' by the same *O*. The corresponding values for *Ro* were 3.15 and 2.65, and for *St*, 3.59 and 2.57. The reliabilities of these differences, as determined in the customary way through calculation of the standard deviations of the means, were 0.95 (*Gl*), 0.94 (*Ro*), and 0.9999 (*St*). If the results of all three *O*s are taken together, the reliability of the difference is greater than 0.9999. The implication is that certain factors which produce agreements *between O*s likewise tend to produce agreement between the selections made by one and the same *O* at different times, and thus are objective in a double sense.

We have already pointed out that the agreements in selections between Series G and H (in which the figures were *different*) were approximately what should be expected by chance, the mean number of agreements being 31.7 out of 100, or 31.7%. The mean number of agreements in the 41 cases falling in both Series H and J was 25.6%, which is somewhat below the chance-value, 33.3%. Both of these empirical values are lower than the mean percentage of agreements between Series G and J (in which the *same* figures were used), which was, as indicated above 49.7%.

We have confirmed in this series our finding in Series H that if the figures are changed, the words selected by the *O*s as names for them tend to change; also we have shown that if the figures are *not* changed, there is marked tendency for *O*s to select the same words as names for the figures, even after an interval as long as 13 months.

Discussion of Results

We have found, as have other investigators, that *O*s agree with each other more markedly than we should expect according to chance when they select one nonsense-word from a list as the best name for a given figure. The results of Series H, in particular, show that these agreements cannot be attributed in general to absolute characteristics of the words alone, and that we must look for bases involving relationship between word and figure.

Earlier investigators, particularly Usnadze, have tacitly assumed that an *O*'s report concerning his experience in choosing a name will reveal the real basis for his choice. Usnadze has classified in four groups the factors underlying naming, without suggesting the possibility that the reported factors are not the real factors. In our own experiments we have been unable, in general, to find a perfect or even a high correlation between actual agreements between *O*s as regards names chosen and the reasons or bases indicated in their reports. All 5 of our *O*s may, as in Series G, agree

upon one word out of three as the best name for a given figure without agreeing in the least upon their bases or reasons for their choice. Here the objection might be raised that the *O*s have a difficult task and that the disagreements between their reported reasons are simply due to omissions or to very imperfect observation. Several considerations aid in disposing of this objection. (1) The *O*s' disagreements in 'reasons' are *positive* disagreements. In some instances, it is true, the *O*s give reports which are simply insufficient, but typically they report positive reasons which disagree from individual to individual. (2) Some of them realized very early in the course of our study that the reasons which they were reporting were simply 'rationalizations,' but were unable in general to draw a strict line between real reasons and such *ex post facto* reasons. (3) There are grounds for believing that 'rationalizations' tend to occur in the full naming-process as contributions to the suitability of the name already selected; the total course of the naming-process, that is, is so closely interwoven that there is a certain artificiality, from the namer's point of view, in dividing the process into distinct stages.

We have found no clear evidence that words 'selected' by intrinsic (*sachliche*) criteria are reproduced more accurately when *O* perceives all the material a second time than are those 'selected' by associative criteria. Our results again conflict with Usnadze's. Unless we assume that his *O*s manifested rare 'insight' into the real grounds for their choices, we must conclude that the discrepancy between our results and his is due primarily to the paucity of his data. We did discover, however, a positive correlation between the number of *O*s who selected a word and the accuracy with which the same word was reproduced when the same *O*s were asked, after a long interval, to find the word chosen before. If high agreement between the *O*s may be traced to certain real bases which are non-associative in nature, the inference would then be that names really selected in terms of such criteria are more likely to be remembered or to be reproduced correctly.

In one respect we were able to determine without difficulty some agreement between real bases and reported bases: certain letters (such as *i*, *z*, and *k*) are more likely to be found in words selected for figures characterized by the *O*s themselves as sharp or angular than in names for round or smooth figures. Other letters (especially *m*, *u*, *l*, and *b*) are more likely to be contained in names for figures characterized as round than in those of the other group. In these cases it would appear that we have certain characteristics of figure and word which are so salient or dominant that they determine not only the actual choice of names but likewise determine the

attributive characterizations reported and possibly elaborated by the observer.

In general, our experiments were not adapted for a detailed study of the real bases underlying naming. The non-correlation between real and reported bases which we discovered so unexpectedly made a systematic study of real bases impossible, except to some extent in the case mentioned just above. Methods such as those of Sapir, Newman, and Bentley and Varon, coupled with studies of the acoustical structure of speech-sounds, are better adapted to the study of the real or objective bases of naming.

According to the most 'obvious' view, the process of selecting a name for an object would be something like this: *O* discovers a word which agrees with the figure in some respect; since there is this agreement, he concludes that the word would be a good name for the object; if he is then asked to report his naming-experience or simply comment to himself, he is able to specify the reasons which led to his choice. According to another view which seems justified and even demanded by our results, the process takes a form such as this: without necessarily knowing 'why' he finds a particular sound-complex has become a name for the object; the word selected is very often announced to him as 'good' or 'bad' or 'mediocre'; he is then able and likely to go on to rationalize the interconnection, either by reading into it certain 'phonetic symbolisms,' by discovering linguistic associations, or both; this enrichment of the relationship between figure and word will then probably be formulated by him as the real basis underlying his choice.

In our study we have unearthed a wealth and variety of attributive characterizations of sound-complexes and have shown that much of this enrichment and elaboration of the naming-function has little or nothing to do with the real basis for the selection of the name. The situation is somewhat the same in actual languages and other codes of symbols. For many persons, the word *cool* not only *means* moderately low temperature but itself *sounds* or even *looks* cool; such words as *melody, massive, breeze,* and *cloud* likewise often bear such 'physiognomies.' But in none of these cases has the physiognomic coloring of the sound-complex *made* it into a name, for the word has taken on a physiognomy only after it has been accepted as a name. Werner has recently emphasized the significance of such physiognomic aspects of words but has been careful to add that the existence of these in developed languages does not authorize attempts to found a general universal physiognomic system (*Urphysiognomik*) for all languages.[55]

[55] H. Werner, Über die Sprachphysiognomik als einer neuen Methode der vergleichenden Sprachbetrachtung, *Zsch. f. Psychol.*, 109, 1929, 337-363.

Conclusions

(1) Individuals agree better than chance would lead us to expect in choosing names for visual figures from lists of nonsense sound-complexes.

(2) With general instructions, *O*s tend to report as reasons for their choices associations involving their knowledge of languages and of familiar physical objects.

(3) Sometimes, under general instructions, but much more often under instructions discouraging reliance upon linguistic knowledge, the *O*s report that they have based their choice upon certain common attributes or characteristics (*e.g.* sharpness, smoothness) of sound-complex and figure.

(4) More rarely, *O* reports simply a 'general impression,' which may or may not be affectively toned, as his basis for the suitability of the sound-complex as a name for the figure.

(5) In general, the *O*s agree upon the same word in a particular case much more frequently than they agree upon their bases for selecting it. It is therefore essential to distinguish between the real bases underlying the choice of the names and the *O*s' reported 'reasons,' which are often simply rationalizations contributing to the suitability of the name already chosen.

(6) There is no significant relationship between type of basis *reported* and the accuracy with which the word is reproduced at a later time when words and figures are re-presented.

(7) There is a positive relationship between the number of *O*s who have chosen a given word and the accuracy of any one of these *O*s in reproducing the word later. Names which have 'objective determinants' in this sense are thus more likely to be reproduced correctly later.

(8) One scale of attributes (*roundness—sharpness*) is so dominant or salient that the real bases and reported bases tend toward agreement. The sounds of *i, z, k,* and certain other letters tend to be sharp or angular; those of such letters as *m, l, u,* and *b,* smooth or voluminous. The *O*s give such characterizations even without any formal instructions suggesting this particular scale of attributes.

OVER-COMPENSATION IN TIME RELATIONSHIPS OF BILATERAL MOVEMENTS OF THE FINGERS

By NEIL WARREN, University of Southern California

An important part of the problem of laterality has been the development of tests for the determination of right- and left-handedness. Some of these tests have been concerned with purely manual activities, such as writing, throwing, and reaching; while some have involved comparisons of other bilateral functions, including the foot used in kicking, or in stepping, the eye used in sighting, etc. The latter group of tests was intended to find whether the laterality evident in handedness involved a functional asymmetry of the organism as a whole. Some of the tests have been measures of obviously learned activities, while others have presumably measured a native handedness which need not correspond to the hand-preference of acquired behavior in any specific activity.

Tests of learned activities. In general the studies of handedness in learned functions have either used the questionary to determine the hand used in performing a number of activities or have observed the performances experimentally.

Handedness' questionaries have been developed by Koch,[1] Jasper,[2] Lauterbach,[3] and Twitmyer and Nathanson.[4] They are designed to indicate degree of handedness. The most common activities observed are writing, throwing, reaching, hammering, batting, sweeping, and shoveling. The strictly right- or left-handed individual is extremely rare.[5] A considerable variation occurs in the proportion of acts performed by each hand. Everyone has many activities which involve the non-preferred hand, as well as many which are ambidextrous.

Tests of native handedness. The term 'native handedness' has been used to mean "the basic laterality of neural organization which may be contra-indicated by laterality of the majority of present manual preferences".[6] Jasper used 'central laterality' in this sense to indicate "an orientational laterality within the more central aspects of the neuro-muscular system with no assumptions regarding its hereditary or ac-

* Accepted for publication June 26, 1935. From the Psychological Laboratory of the University of Southern California. This study was performed under the direction of Dr. Milton Metfessel.

[1] H. L. Koch, et al. A study of the nature, measurement, and determination of hand perference, *Genet. Psychol. Monog.*, 13, 1933, 117-221.

[2] H. H. Jasper, A laboratory study of diagnostic indices of bilateral neuromuscular organization in stutterers and normal speakers, *Psychol. Monog.*, 43, 1932, (no. 194), 72-174.

[3] C. E. Lauterbach, Shall the left-handed child be transfered? *Ped. Sem.* 43, 1933, 454-462.

[4] E. B. Twitmyer and Y. Nathanson, The determination of laterality, *Psychol. Clinic*, 22, 1933, 114-148.

[5] Cf. J. M. Rife, Types of dextrality, *Psychol. Rev.*, 29, 1922, 474-480.

[6] Jasper, *op. cit.*, 77.

quired nature."[7] Most users, however, of the term 'native handedness' assume its hereditary nature although the concept is still in dispute.

The principal basis for the development of tests of native physiological preference for handedness has been the apparent relationship between laterality and speech-defect. One theory, assuming this relationship, holds that the cortical center controlling speech in right-handed individuals is located in the left hemisphere; if a sufficiently dominant gradient is not established in one hemisphere, or if it is interfered with by change of handedness, stuttering is alleged to follow. One proponent of this theory is Travis, who writes:

> The principle of cerebral dominance, the clinical application of which has been so efficacious, posits a physiological substratum in the determination of right laterality of motor leads. It conceives as existing a center of chief dominance in the left cerebrum which exerts a dynastic influence over the right cerebrum to insure concerted but unequivocal action. When there is no sufficiently dominant gradient of activity present in either hemisphere, or when such is resident but not heeded in the right hemisphere, what I conceive to be the patho-physiological subsoil of the essential speech dysfunctions obtains.[8]

Both diagnosis and determination of therapeutic measures for this theory require the use of tests to indicate the native handedness of the stutterer. In order to avoid the direct effects of learning, a number of tests have been devised to measure bilateral performances in unpractical activities. In such tests as mirror-tracing, it is intended that both hands should be placed on an indentical basis since neither has been specifically trained. Tests of eyedness in sighting, fixating, etc., measure functions of which the individual is seldom aware, and therefore, are presumed to be bilaterally determined by a central neural bias.

Other measures used have been relative strength of grip of the hands, tapping, steadiness, orientation in simultaneous writing with both hands, and structural asymmetries. While none of these tests measure the direct results of learning, they may be influenced indirectly by the relative use of the hands in learned activities. In the following pages, the results of the present study will show a definite bearing upon the doubtfulness of any alleged "test of native handedness."

Prior response in simultaneous movements. The relative precedence of the right or left hand in attempted simultaneous movement has been used in a number of studies as an indication of "native handedness." In general, these studies agree in showing prior movements by the non-preferred hand.

Travis and Herren studied the temporal relationship of the hands in attempted simultaneous antitropic movements from the shoulder.[9] The subject was instructed to move both hands in opposite directions at the same time. A record of the initial impulse of each hand was made on the kymograph-drum. Right-handed normal speakers, with a history of right-handedness, were found to lead more frequently with the left hand; left-handed normal speakers, with a history of left-handedness, lead more frequently with the right hand; presumably right-handed stutterers, regardless of history of handedness, lead more frequently with the right hand. Using a similar technique, Travis and Bryngelson found a definite tendency toward a

[7] *Ibid.*, 78.
[8] L. E. Travis, *Speech Pathology*, 1931, 14.
[9] L. E. Travis and R. Y. Herren, Studies in stuttering: V. A study of simultaneous antitropic movements of the hands of stutterers, *Arch. Neur. & Psychiat.*, 22, 1929. 487-494.

greater number of right-hand leads by stutterers than by right-handed normal speakers in adductive movement.[10]

Jasper classified stutterers definitely with ambidextrous or left-handed normal speakers, rather than with dextrals. In a measurement of the prior movements of the forearms in attempted simultaneous movement, he found that right-handed normal speakers lead more frequently with the left hand; stutterers and sinistrals lead more frequently with the right hand. In movements of the digits there was no differentiation between the groups of *Ss*, all groups showing more right than left leads.[11] Greenberg,[12] taking reaction-times, found a tendency for prior movements by the non-preferred hand.

Orton and Travis attempted to secure a measure of the native physiological preference in handedness by recording the neuro-muscular action currents in simultaneous voluntary contraction of the fingers of the two hands. The study used a group of normal speakers and a group of stutterers. The authors reported that:

> the precedence pattern of the action currents in stutterers who use the right hand by preference in trained motor acts varies strikingly from that of normal speakers who have the same right hand preference. . . . In records of right-handed normal speakers, the number of times that the action currents precede in the right arm far exceeds the precedence in the left, and also far exceeds the incidence of simultaneous leads.[13]

Metfessel and Warren secured both action currents and movement leads on the same record and reported that the hand not used in writing was most likely to precede in both movement and action currents.[14] Because of the correspondence between the two measures, movement leads were held to be as significant as action currents in relation to handedness. In both action-current leads and movement leads, stutterers and dextro-sinistrals resembled dextrals more than sinistrals. There was, however, a significant difference between dextrals and sinistrals in both measures.

Preliminary Study

As a check upon the tendency toward prior movements by the non-preferred hand, data were secured from 100 *Ss*, 65 right-handed and 35 left-handed writers. They were tested for movement leads only. No attempt was made to record the action-currents.

Apparatus and procedure. A simple device was prepared to indicate the prior movement by means of the flash of a neon light.[15] The data recorded were in terms of the number of times one key was pressed before the other. The *Ss* attempted to press two telegraph keys simultaneously each time a signal light flashed. They were instructed to rest the first two fingers of each hand upon the keys and to

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[10] See Jasper, *op. cit.*, 85. [11] *Ibid.*

[12] A. D. Greenburg, The simultaneous reaction-time of the two hands of stutterers during vocalization, (M.A. thesis), *Univ. Southern Calif.*, 1930.

[13] S. T. Orton and L. E. Travis, Studies in stuttering: IV. Studies of action currents in stutterers, *Arch. Neur. & Psychiat.*, 21, 1929, 68.

[14] M. Metfessel and N. Warren, Over-compensation by the non-preferred hand in an action-current study of simultaneous movements of the fingers, *J. Exper. Psychol.*, 17, 1934, 246-256.

[15] The apparatus will be described with the main experiments.

move them sharply to the rhythm of the signal light. *E* recorded each pressure by means of a counter for each hand.

At least 100 trials were recorded for every *S*. All but 15 of the 100 *S*s lead consistently with one or the other hand. The results are listed in Table I.

In order to determine the effect of increased disparity between fingers involved, a comparison was made of the relative movements of the following combinations of fingers: (1) the index finger of the left hand and the little finger of the right, and (2) the index finger of the right hand and the little finger of the left. A group of 31 left-handed writers and 15 right-handed writers was given this test. The results are given in Table II.

TABLE I

MOVEMENT LEADS OF THE FIRST TWO FINGERS OF EACH HAND

Handedness	Left-hand leads		Right-hand leads		Indeterminate leads	
	No.	%	No.	%	No.	%
Right	42	64.6	15	23.1	8	12.3
Left	11	31.4	17	48.6	7	20.0

TABLE II

RESULTS OF INCREASED DISPARITY IN FINGER USE

Handedness	Little finger of right hand and index finger of left hand					
	Little-finger leads		Index-finger leads		Indeterminate leads	
	No.	%	No.	%	No.	%
Right	7	46.7	6	40.0	2	13.3
Left	16	55.2	8	27.6	5	17.2
	Index finger of right hand and little finger of left hand					
Right	13	86.6	1	6.7	1	6.7
Left	20	64.5	5	16.1	6	19.4

Discussion of results. The results of the measurement of priority of right- or left-hand movements indicate the tendency for the non-preferred hand to move first. It will be observed in Table I that the tendency is more pronounced in the case of right- than of left-handed individuals. It seems likely that this is due to the fact that most left-handers make more use of the right hand than right-handers of the left. For the total group of 100 *S*s the non-preferred hand led consistently in 59 cases, the preferred hand in 26 cases, and in 15 cases neither hand led consistently.

The results of the experiments which involve the relative movements of the forefinger of one hand and the little finger of the other indicate that the cause of prior movements is not to be found in *S*'s laterality but, rather, in the imbalance between the two sets of muscles being used (Table II). For the total group, the little finger of the non-preferred hand consistently led the forefinger of the preferred hand 29 times; the forefinger led 9 times, and in 6 cases neither finger led consistently. The little finger of the preferred hand led the forefinger of the non-preferred hand 27 times, followed it 11 times, with no definite lead in 8 cases. The slightly greater proportion of leads favoring the little finger of the non-preferred

hand is presumably due to the greater disparity of use between it and the forefinger of the preferred hand than between the members of the opposite combination.

The terms 'more-used' and 'less-used' will be employed in the remainder of this study to indicate a relationship between the fingers which seems better described in terms of use than in such structural terms as 'stronger,' 'weaker,' etc. In manual activities the fingers of either hand are used to about the same extent. In digital activities, however, a definite difference in use between fingers is observable. In writing, prehension, etc., the little fingers are only slightly used. Of the four fingers which are compared in this study, the fore-finger of the preferred hand is obviously most used. The forefinger of the non-preferred hand is somewhat less used since it is not involved in the writing process. The relationship between the little fingers, in regard to use, need not be considered, since, in this study, they are compared with the forefingers of the opposite hand and not with each other.

Main Experimental Procedures

The three phases of the main experimentation may be listed as follows: (1) relative movement of paired fingers in attempted simultaneous reaction; (2) the effect on weight discrimination of the particular finger used in lifting the weights; and (3) relative finger pressures. In each experiment, three combinations of fingers are studied: (a) the forefinger of each hand; (b) the forefinger of the left hand and the little finger of the right hand; and (c) the little finger of the left hand and the forefinger of the right hand.

In these experiments 20 right-handed and 20 left-handed *S*s were used. The determination of handedness was made only on the basis of writing hand, no *S*s being included in either classification who might be definitely classed as ambidextrous. None had any extended previous experience in any of the procedures involved.

(1) Relative priority of movement. The measurement of the priority of finger movements when it is intended that the two fingers shall be moved simultaneously is an extension of the preliminary experiments already reported. Since the experiments of Metfessel and Warren indicated the same significance of actual movements and of the action currents accompanying the movements, no attempts were made to record the action currents in the present study.

In comparing simultaneous antitropic movements of the hands, Travis and Herren used a high-speed kymograph on which the movements were recorded mechanically. Travis and Bryngelson modified this technique by using two break keys which were connected in a bridge circuit to a milliammeter. The precedence of movement of the right key would cause a deflection of the milliammeter. Jasper's apparatus involved the movement of two telegraph break keys placed in the opposite arms of a resistance balanced Wheatstone bridge circuit. The breaking of either key would cause a deflection of the galvanometer toward the side indicated. If the keys were broken at the same time, no deflection would occur, but the apparatus was so sensitive that such occurrences could be disregarded. The extent of the temporal lead was indicated by the amount of deflection of the galvanometer mirror as shown on a large scale.

The apparatus used in the present study to determine the precedence of finger movements was similar to that used in the action-current study described by Metfessel and Warren.[16] The fingers rested upon telegraph keys mounted on heavy wooden blocks. A movement of the fingers toward the body served to depress the keys. Any movement would open the electrical circuit in which the key was placed. Each key was adjusted to require a pressure of 300 gm. to break the circuit, and also to allow a movement of only 0.5 mm. when it was depressed. *S* was requested to permit his fingers to rest lightly upon the surface of the keys at all times, even when not depressing them. The arms rested on pads to increase comfort.

A green signal[17] was mounted in front of *S*, approximately on a level with his eyes. The light, using a 6-volt current, flashed at a constant rate of 45 flashes per min. The speed of the recurring flashes was determined by a contact operated by a phonograph motor.

The telegraph keys were placed in a circuit so arranged as to indicate by means of the flash of a neon glow lamp which key was pressed first. When one key (A) was pressed first the lamp would light; when the other (B) moved first the lamp remained unlighted. The neon lamp is practically instantaneous as shown by its successful use in high frequency stroboscopy in which a response within an interval as small as 0.00001 sec. is required. Absolute simultaneity of movement is practically impossible and any variation therefrom favoring key A would be sufficient to cause the neon lamp to flash.[18] The light indicating the response was concealed from *S* by a screen.

S was instructed to press the keys in the following way:

Sit near enough to the table so you can easily reach the telegraph keys. Place the forefinger of each hand on the key, permitting it to rest lightly at all times. You are to press both keys at the same time, each time the signal light flashes. It will flash at the rate of about once a second.

You need not press the keys as hard as you can, but make each movement a quick, sharp jerk rather than a slow pressure.

The apparatus did not, of course, indicate the extent by which one hand preceded the other. It merely indicated which key had been pressed first. If the light flashed, the hand on key A had moved first; if the light did not flash, the other hand had preceded. *E* recorded the number of flashes and the number of times the keys were pressed in which the light did not flash. In order to check the apparatus, the keys were placed in one position for one-half of the responses and were changed to the other hand for the other half. An approximately equal proportion of responses by the same hand in both positions indicated the reliability of the apparatus.

Approximately 80 trials were given to each *S* for each combination of fingers studied. The results are stated in terms of the percentage of times one hand preceded the other. That the significance of the measure is equivalent to the average extent is indicated by a positive correlation of 0.89 $\pm$ .01 between the extent by which the

[16] M. Metfessel and N. Warren, *op. cit., J. Exper. Psychol.*, 17, 1934, 246-256.

[17] The light was a Dunlap visual stimulus purchased from C. H. Stoelting and Company, Chicago, Ill.

[18] The action-current study reported by Metfessel and Warren (*op. cit.*) indicated that attempted simultaneous movements occurred within 0.003 sec. of each other only 26.1% of the time, and within 0.001 sec. only 11.0%.

leading hand preceded the other and the percentage of prior movements in the preliminary studies.

(2) Weight discrimination. The method of equivalents as used here involves adjusting the intensity or extensity of a pair of stimuli in such a way that they may be perceived as equal when received by different sense organs. It is possible to vary the objective relationships of the stimulus-objects in such a way that they are reported equivalent. The amount by which one stimulus must be increased to make it subjectively equal to the other may be termed a measure of the subjective increment produced by the sense organ stimulated by the latter. That is, if a weight of 80 gm. lifted by the left hand is judged to be equal to 84 gm. lifted in the right hand, it may be said that the left hand is subjectively increased by 4 gm. The subjective increment may be expressed in actual amount or in terms of percentage of a standard stimulus used in comparison.

The weights used in the present experiments were described by Whipple.[19] They consist of a standard of 80 gm. and 25 comparison weights all heavier than the standard. Their weights are 80.5, 81, 81.5, 82, 82.5, 83, 83.5, 84, 84.5, 85, 86, 87, 88, 89, 90, 92, 94, 98, 100, 105, 110, and 120 gm. They are made of wood cylinders, 3 in. tall and 1 in. in diam., and are weighted with lead. Since, in these experiments, the weights were not handled by *S*, nor seen during the test, shape, size, and color were of no consequence.

In order to permit the judgment of the weights in lifting them by single fingers, holders were prepared to suspend the weights from the fingers. The holders were made of fabric-covered copper wire and were identical in shape and weight. They weighed exactly 5 gm. each. This weight was taken into account in the derivation of the limens for the various tests.

S was seated in front of *E* in an arm-chair, the arms of which were padded with pillows to serve as rests for *S*'s forearms. The following instructions were read to him:

> Take a comfortable position with your arms resting on the pillows. Keep the palms of both hands upward with the fingers slightly extended.
>
> I am going to suspend weights from various fingers and you are to tell me which seem to be the heavier. The differences will be small but in no case will the weights be the same. You are to respond by saying 'right' or 'left,' as the case may be, indicating heavier weight.
>
> Make your response as soon as you can. Your first impression is probably the best. It will be necessary, of course, for you to keep your eyes closed during the test. We will begin by using the forefinger of each hand.

In order to keep the conditions constant, the weights were hung at the joint between the first and second phalanges of each finger; *E* attempting to release each weight at the same time. The weights were presented by a rhythmic movement with pauses at the end of each 5 trials for recording, and at the end of each 25 trials to permit *S* to rest. On the average 100 to 150 trials were required to establish the limen; the number depending upon the range of the interval of uncertainty.

In beginning the experiment, the 100-gm. weight was first compared with the standard and the difference was decreased rapidly to determine the approximate limen. Thereafter the comparison weights were varied only between one sufficiently great to secure 80% or more correct responses, and one small enough to cause 80% or more incorrect judgments. In a few cases the limen was found to be greater than

[19] G. M. Whipple, *Manual of Mental and Physical Tests,* 1910, 190.

100 gm. Approximately 20 trials were made for each of the comparison weights studied. *S* was required to make a judgment of 'heavier right' or 'heavier left' in each trial and to guess if he were unable to judge otherwise.

Derivation of data. In determining the limen for each subject the method of linear interpolation described by Brown and Thomson was used.[20] This method is the simplest of the procedures for calculating the limen from data secured by the method of constant stimuli. In addition to its simplicity it has the advantage of dispensing with the assumption that the data fit the normal curve. Newhall has recommended the use of interpolative methods in such studies as the present rather than the more extended computations of the constant process.[21]

In its simplest form the method of interpolation involves determining the weight which results in 50% correct responses by interpolating between the weight giving more than 50% and that giving less than 50% correct responses. If Q_2 is the desired weight;[22] D_1 the weight giving more than 50% correct judgments; D_2 the weight giving less than 50% correct responses; P_1 the percentage of correct judgments for D_1; and P_2 the percentage of correct responses for D_2; the formula is

$$(D_1-Q_2)/(Q_2-D_2) = (P_1-50)/(50-P_2) \quad [1]$$

In this form only two of the percentages are used. Thomson,[23] however, extended the method by use of those amounts which give 75% (Q_3) and 25% (Q_1) correct responses. These figures give also a measure of the scatter of the data. If D_3 is the weight which results in P_3, a percentage greater than 75; and D_4 is the weight giving P_4, a percentage less than 75; then Q_3 may be determined by the formula:

$$(D_3-Q_3)/(Q_3-D_4) = (P_3-75)/(75-P_4) \quad [2]$$

Q_1 may be found similarly by the following formula:

$$(D_5-Q_1)/(Q_1-D_6) = (P_5-25)/(25-P_6) \quad [3]$$

in which D_5 is the stimulus giving P_5, a percentage of correct responses greater than 25; and D_6 is the stimulus giving P_6 a percentage of correct judgments less than 25.

The threshold T is equal to the average of the results of formulas (1), (2), and (3), or

$$T = (Q_1 + Q_2 + Q_3)/3 \quad [4]$$

(3) Relative pressure. The three pairs of fingers were compared for pressure exerted by each finger when the attempt was made to press two keys with equal force and simultaneously.

An apparatus was devised by which the pressure of each finger upon a specially built key could be recorded upon a kymograph drum. The keys were mounted similarly to those described in the previous section. A contraction of the fingers pressing the keys toward the body of *S* operated a stylus which recorded the extent of the movement upon a revolving drum. The depression of the keys compressed

[20] Wm. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1925, 224.

[21] S. M. Newhall, Linear interpolation *vs.* the constant method, this JOURNAL, 38, 1927, 390-402.

[22] Brown and Thomson use the symbol T to refer to this figure as well as the derived limen (see formula 4).

[23] G. H. Thomson, A comparison of psychophysical methods, *Brit. J. Psychol.*, 5, 1912, 203-241.

two coil springs of equivalent tension. An adjustment screw varied the compression of each spring so as to balance the two keys exactly. They were calibrated simultaneously to secure a displacement of the writing points of the stylii of one millimeter for each 100 grams pressure.

The instructions were as follows:

You are to press the keys at the same time with the pairs of fingers we have been studying. Try to press equally hard with each finger. Begin pressing when I say 'Go,' and hold the keys with the same pressure till I say 'Stop.'

The kymograph was started and allowed to revolve once to produce a base line. The subject was then told to begin pressing the keys and to hold them for 15 sec., giving a recording of 15 centimeters on the kymograph paper. The record was measured in terms of millimeters displacement at each of 10 successive centimeter intervals (corresponding to 10 successive sec.) of the recording, starting 1 centimeter after the initial movement had been completed. The millimeter displacement was later translated into grams pressure. The data secured were the averages of the ten measures in terms of grams of pressure upon the keys.

Experimental Results

The results of the three procedures will be given separately to point out the significant facts of each, and a comparison of the results will be made to determine the relationship between them.

(1) Relative priority of movement. (a) Precedence of the less-used finger. As a whole the results of this study correspond closely to those of the preliminary experiments. The less-used finger was found to precede the more-used one in 80, or 66.7% of the 120 cases. The more-used finger preceded in 19, 15.8% of the cases, and in 21, or 17.5%, there was no consistent lead by either finger.[24] The summary of the results of this experiment in Table III shows that here also the greater the disparity of use between the fingers the more likely was the less-used to precede the more-used finger. The greatest tendency for this to occur was in the case of the little finger of the non-preferred hand in its use with the forefinger of the preferred hand.

(b) Consistency of the leading hand. In 30 cases, measures of relative movements were repeated on different occasions, some as much as two months apart. In 23 of these repeated experiments, the same finger lead in both measures. In 5 of the remaining 7 cases, the percentage of one or both tests was indeterminate. A correlation of the percentages of leads in the repeated tests of these 30 *Ss* resulted in a positive coefficient of $0.658 \pm .066$. One dextral *S* was given on one occasion a total of 400 trials with the forefingers, with the percentage remaining virtually unchanged at the end of each 100. The percentages of left forefinger leads were 76.0, 72.0, 77.0, and 80.0. In view of these facts it is concluded that the tendency for one finger to precede consistently, when it is intended that the two shall move simultaneously, is constant both in direction and in degree.

[24] In order to be significant, the percentage of leads must differ reliably from 50%, which would indicate a purely chance order of the movements of the fingers. The formula used to derive the probable error of the percentage was $P.E. = .6745pq/N$; in which p is the percentage, q is its reciprocal, and N is the number of cases. See L. E. Travis, A comparative study of the performances of stutterers and normal speakers in mirror tracing, *Psychol. Monog.*, 39, 1928, (no. 178), 45-50.

(c) Effect of fatigue. The effect of fatigue upon the relative movements of the fingers was to cause a consistent precedence of the fatigued finger. In order to determine this fact the forefinger of each hand was fatigued at different times and *S* was required to attempt to press with the fingers simultaneously. The pressure keys were used as ergographs to produce the fatigue, *S* required to press repeatedly with the selected finger for a period of 30 sec. just before the test of relative movements. He was instructed to press as hard and as fast as possible. After testing the right hand for relative movements, an interval of at least 30 min. was allowed before fatiguing the left hand. In each case the fatigued member led consistently over the non-fatigued finger.

(2) Weight discrimination. (a) Overestimation by the less-used finger. In each pair of fingers compared, the weight lifted by the less-used finger was judged heavier

TABLE III

SUMMARY OF RELATIVE PRIORITY OF MOVEMENT

	Sinistrals			Dextrals		
	No.	%	Av. lead (%)	No.	%	Av. lead (%)
Left forefinger	5	25		12	60	Left, 65.6
Right forefinger	10	50	Right, 58.4	4	20	
Indeterminate	5	25		4	20	
Left forefinger	3	15		6	30	
Right little finger	16	80	Right, 79.9	9	45	Right, 51.2
Indeterminate	1	5		5	25	
Left little finger	14	70	Left, 70.4	19	95	Left, 79.0
Right forefinger	1	5		0	0	
Indeterminate	5	25		1	5	

in a significant majority of the cases. Of the 120 limens determined by this method, 96 (80%) indicated a greater estimation of the weight lifted by the less-used finger; 11 (9.2%) by the more-used finger; and 13 (10.8%) indeterminate. This last group included all cases in which there was no significant difference between the two fingers involved. The complete results for both right- and left-handed groups and for all three combinations of fingers are summarized in Table IV.

In the comparison of the forefingers of the hands it is to be noted that the non-preferred hand was consistently overestimated in relation to the preferred hand. That this is not a matter of the laterality of the individual is indicated by the fact that the little finger of the preferred hand was overestimated in comparison to the forefinger of the other hand.

The greater the disparity between the fingers in their use, the more definite was the difference in weight discrimination. The little finger of the non-preferred hand and the forefinger of the preferred hand produced the greatest differences in weight judgments, while the little finger of the preferred hand and the forefinger of the non-preferred hand resulted in the smallest differences.

(3) Relative pressure. The measurement of relative pressure of the fingers has not given as consistent a tendency as the measures of weight judgment and relative movement. The actual pressure of the fingers varied considerably among *S*s; in some cases one finger pressed several times as hard as the other when the intention

TABLE IV

SUMMARY OF WEIGHT DISCRIMINATION

	Sinistrals			Dextrals		
	No.	%	Av. increment (gm.)	No.	%	Av. increment (gm.)
Left forefinger	1	5		17	85	Left, 8.1
Right forefinger	15	75	Right, 9.1	2	10	
Indeterminate	4	20		1	5	
Left forefinger	1	5		5	25	
Right little finger	17	85	Right, 15.8	12	60	Right, 2.8
Indeterminate	2	10		3	15	
Left little finger	16	80	Left, 7.0	19	95	Left, 18.1
Right forefinger	1	5		1	5	
Indeterminate	3	15		0	0	

was to press equally with both. In 21 cases (17.5%) the finger which exerted greater pressure, pressed less than 10% harder than the other. These cases were considered indeterminate. The others indicate a definite tendency to press harder with one or the other finger. In 47 (39.2%) of the cases, the less-used finger pressed harder. In 52 (43.2%) of the cases, the greater pressure was made by the more-used finger. In this group an insufficient allowance was made for the inferior member in the attempt to equalize the pressure. In the former group two explanations

TABLE V

SUMMARY OF RELATIVE FINGER PRESSURES

	Sinistrals			Dextrals		
	No.	%	Av. greater pressure %	No.	%	Av. greater pressure %
Left forefinger	8	40	Left, 9.1	7	35	Left, 12.1
Right forefinger	5	25		8	40	
Indeterminate	7	35		5	25	
Left forefinger	11	55	Left, 4.6	8	40	
Right little finger	7	35		10	50	Right, 17.0
Indeterminate	2	10		2	10	
Left little finger	9	45	Left, 2.6	9	45	
Right forefinger	7	35		10	50	Right, 4.2
Indeterminate	4	20		1	5	

may be given for the greater pressure by the less-used member: (1) the sensory processes may not be so great; (2) the attempt to allow for the inferiority of the member may result in over-compensation. That both of these factors are operative is indicated by various facts to be discussed in the next section.

A summary of the data for this test is given in Table V.

(4) Intercorrelation of measures. A study of the interrelationships of the various results indicates a bearing on the following points: *(a)* The greater the sensory imbalance, as measured by weight-discrimination, the

greater was the tendency for the less-used finger to precede in attempted simultaneous movements. *(b)* The relative amounts of pressure exerted by a pair of fingers, when it is intended that they should press equally hard, is affected by the sensory imbalance between them. *(c)* The greater tendency to allow for the inferiority of a finger by pressing relatively harder with it is accompanied by an increased tendency to move that finger earlier in attempted simultaneous movements.

(a) Correlation of weight discrimination and relative movement. In 77.5 ± 2.6% of the cases the finger which is relatively overestimated in weight judgment also moves earlier. There is a positive correlation of 0.524 ± .045 between the amount of subjective increment and the percentage of leads in each case. From these facts

TABLE VI

WEIGHT DISCRIMINATION IN RELATION TO FINGER PRESSURE

Group	Cases of greater pressure by less-used finger		Cases of smaller pressure by less-used finger	
	Weight judged heavier on	Subjective increment (gm.)	Weight judged heavier on	Subjective increment (gm.)
Dextrals	Less-used finger	7.6	Less-used finger	11.1
Sinistrals	Less-used finger	8.7	Less-used finger	12.2
Total	Less-used finger	8.1	Less-used finger	11.7

TABLE VII

RELATIVE PRESSURE AND RELATIVE MOVEMENT

Group	Cases of greater pressure by less-used finger		Cases of smaller pressure by less-used finger	
	Leading finger	% leads	Leading finger	% leads
Dextrals	Less-used finger	66.3	Less-used finger	62.7
Sinistrals	Less-used finger	67.1	Less-used finger	63.2
Total	Less-used finger	66.7	Less-used finger	63.0

it may be concluded that the priority of the less-used finger of any pair is probably due to a perception of inferiority of that member.

(b) Weight discrimination and relative pressure. No significant correlation was found between the amount of greater pressure and the relative estimation of weights lifted by the fingers. The coefficient of correlation was +0.084 ± .058, with 49.1% of the cases indicating the same finger in each measure. A slight relationship was found, however, in the fact that the cases in which the less-used finger exerted greater pressure had a smaller average increment by that finger in weight discrimination than in those cases in which the less-used finger exerted less pressure. That this was true for both dextrals and sinistrals is shown in Table VI. It is evident that the greater sensory response is a limiting factor in the cases in which the less-used finger exerts less pressure.

(c) Relative pressure and relative movement. That there is no significant correlation between the amount of pressure by the less-used finger and its tendency to precede in 'simultaneous' movement is indicated by the coefficient of —0.065 ± .061. In 55% of the cases the finger which pressed harder was slower to respond

in the test of relative movement. Again, however, a relationship between the two measures is shown by the fact that those cases in which the less-used finger over-compensated in relative pressure had a greater proportion of leads by that finger. The averages are given in Table VII. The differences seem to indicate that the tendency to over-compensate by prior movements of the less-used finger was more definite in the cases which over-compensated by pressing harder with the less-used member.

The Bearing of Over-Compensation on Tests of 'Laterality'

The results of the experiments just described may be discussed from two points of view. *(1)* They may be examined to determine their bearing upon the phenomena of compensation; and *(2)* they may be considered in their relation to the problem of laterality. From the first point of view may be discussed the tendency for the less-used fingers to precede the more-used in attempted simultaneous movement, and in some cases, the tendency to press harder with the less-used fingers. Under the second point may be included the relative reactions of dextral and sinistral groups.

An adequate test of so-called 'native handedness' would of necessity be uninfluenced by the use of the hands in learned activities. No test which is directly affected by the increased facility of function or the increased muscular development which accompanies greater use of one hand or the other could be held to indicate a native physiological preference. A measure of skill in writing, or of strength of grip, therefore, would not indicate 'native handedness.' It is evident, moreover, that present preferences resulting from admittedly learned manual activities may exert *indirect* influences upon tests presuming to measure native laterality. Of such purported tests, the precedence of response in bilateral movements is considered in the present study. The indirect effect of learned activities upon this test seems to involve a factor of over-compensation.

(1) Over-compensation. In the most common meaning of the term, 'compensation' is the process of overcoming a perception of inferiority which may accompany a deficiency either real or imaginary. The process may be 'direct' in that the individual may try to improve in the trait in which he has felt inferior, or it may involve a substitution of attempted superiority in another trait. In either case the individual is seldom satisfied to achieve an average degree of excellence in the trait; his satisfaction requires that he become superior in the same trait or in the substitute. In this sense the process is usually one of over-compensation, of making a greater allowance for the defect than is objectively required. This type of compensation is a psychological adjustment frequently called a defense mechanism.

It has been pointed out by Vaughn that there are numerous examples of physiological compensation.[25] A leaky valve of the heart may result in a compensatory increase in the strength of the cardiac muscle. The increased strength of one lung or of one kidney in cases of injury to the other organ may be thought of as physiological compensation. The process need not be a conscious attempt to strengthen the weak organ or to improve some vicarious function. The inferiority may result

[25] W. F. Vaughn, The psychology of compensation, *Psychol. Rev.*, 33, 1926, 467-479.

in increased activity and consequent greater strength without the individual being aware of the change. This process may be thought of, also, as over-compensation since it usually results in a development greater than required merely to overcome the deficiency. Adler has said on this point that, "often a morphologic or functional deficiency of an organ is converted to a higher development of that organ."[28]

The precedence of the less-used finger is probably best explained as a form of over-compensation. In the attempt to press the two keys at the same time, a sufficient allowance may be made for the relative inefficiency of the less-used member to cause the movements to vary only slightly and in chance order. If, however, too great an allowance is made, a consistent temporal over-compensation will appear. This may be the result of two causes: *(a)* a greater weakness, either actual or subjective, of the less-used finger; *(b)* a greater tendency to allow for the perceived inferiority. In the present experiments there are indications that both factors are involved.

(a) Greater tendency to over-compensate in cases of greater inferiority. Two facts indicate that there is a more definite priority of the less-used finger if it is perceived to be weaker. First, the greater the disparity between the fingers the more decided is the tendency. It may safely be assumed that the forefinger of the preferred hand is the most frequently used of the fingers studied. The little finger of the non-preferred hand is the least used. The greatest tendency for over-compensation was found in the comparison of these two figures in both dextral and sinistral groups. The more pronounced difference on the part of the right-handed group probably indicates a smaller difference in use between the hands on the part of the sinistrals. In view of the decidedly right-handed organization of modern life left-handed individuals have more opportunities for use of the non-preferred hand than do dextrals. Both the tests of relative movement and of weight discrimination, as well as the preliminary tests, show a smaller difference between the hands in the case of sinistrals.

The second indication that over-compensation is the result of a perception of inferiority of the less-used finger is found in the correlation of the amount of subjective difference between the fingers in weight lifting and the degree to which one precedes the other. This point is shown further by the fact that the average percentage of leads by the 20 cases which had a subjective increment of 20 gm. or more, was 80.5, while the average lead by the less-used finger in all cases was 66.1%.

(b) The tendency to over-compensate in different functions. No attempt was made to investigate the tendencies of the various Ss to overcompensate in activities other than the two motor functions measured in this study. The relationship between the two, however, is such as to indicate that the over-compensation in pressure tests means a greater tendency to over-compensate in relative movement. The 58 cases in which the less-used finger pressed harder, lead with that finger 66.7% of the time. The 60 cases of less pressure by the less-used finger lead with that finger 63.0% of the time.

In view of these facts we concluded that the order of finger movements in attempted simultaneous flexion is definitely influenced by the tendency to over-compensate for the perception of inferiority of the less-used finger.

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[28] A. Adler, *Organ Inferiority and Its Psychic Compensation*, 1917, 22.

(2) Laterality. (a) Prior movement by non-preferred hand. In both the preliminary studies and the main body of the experimentation, the results were definitely different for dextrals and sinistrals, except for the results of measurement of relative pressure. In the study of relative movements there was a consistent tendency for the hand not used in writing to respond earlier when the forefinger, or the first two fingers, of each hand were compared. In the significantly larger part of each group the individuals led consistently with the non-preferred hand. This is true in all measures of movement alone and in the earlier tests of action-current precedence.

The measure of relative movement cannot be considered an adequate test of handedness, however. There are, among both dextrals and sinistrals, some individuals whose results are definitely contrary to rule. The *S*s selected for this experiment were definitely left- or right-handed. A total of 9 of the 40 cases led consistently with preferred hand while 22 *S*s led with the non-preferred hand. While related to handedness, the method is obviously not a reliable test of laterality.

In addition to the predominant lead by the non-preferred hand there are three results of the present experimentation which indicate that priority of hand movements, and of action-currents, cannot be said to be an index to 'native laterality.' (1) In a preliminary series of experiments the group of dextro-sinistrals, all using the right hand in writing but most with a history of left-handedness, was not significantly different from the group of dextrals but differed definitely from the sinistrals. (2) The precedence of the fingers in attempted simultaneous movement was found to be affected by the amount of use which the members received. The greater the difference between the fingers, the more definite the effect. The priority is not dependent upon laterality of the individual since it may be changed from one hand to the other by selection of the fingers involved. (3) The precedence of the fingers in attempted simultaneous movement is affected by the relative fatigue of the members. A consistent precedence by the finger experimentally fatigued was found in the case of either right or left hand.

(b) Relative pressure of the two hands. From the data in Table V it is clear that no significant laterality factor can be found in the relative pressure exerted by the fingers of right or left hands. There is a slight, but not significant, tendency to press harder with the preferred hand in the various combinations of fingers. In any case, the amount of pressure seems dependent upon the degree to which allowance is made for the sensory imbalance between the fingers rather than to laterality. Since the process is not a measure of the strength of the fingers involved, the amount of pressure is dependent upon factors of discrimination and compensation.

(c) Tests of laterality in general. The results of the present experimentation bring into question the value of tests which are intended to indicate a native-handedness preference apart from the influence of learning. Assuming that many individuals have developed right-handed habits in spite of an original left-handedness, it has been held that a physiological factor of laterality remained relatively unchanged. Various tests have been used to measure such a native factor. The results of most of them, however, are subject to the same influences which have been found to operate in the present study. Tests of the relative performances of the two hands, if not directly affected by muscular development and skills resulting from training, may be influenced by the tendency to compensate for muscular inefficiency. That this was true of the action-current study reported by Metfessel and

Warren indicates that neurological tests may also be subject to the same question.

(d) Sensory function of the non-preferred hand. Recent studies have been made to determine the relative sensory value of the two hands. Smith reported a greater tendency to use the non-preferred hand in sensory activities, such as solving the form board tests.[27] In work with the blind she found a "conspicuous superiority of the left hand as a sensory organ."[28]

The present experiments bear upon this problem to the extent that the greater sensory function of the less-used hand in weight discrimination tests may indicate its greater sensory value in other activities. On any test which involves lifting an object for examination, the object may be perceived as heavier when lifted by the non-preferred hand. The experimental results do not indicate anything with regard to other forms of sensitivity. It may be pointed out, however, that an accumulation of callous tissue on the more-used hand will result in decreased sensitivity, but this cannot be attributed to any laterality of sensory functions.

Summary and Conclusions

The purpose of this study was to examine critically the significance of prior movements of the fingers as an indication of an alleged 'native handedness.' Experiments were conducted to determine the precedence of the movements of the fingers of the right and left hands in attempted simultaneous movements, and to consider the bearing of over-compensation and of laterality upon the order of movement.

The mechanism of over-compensation operates in attempted simultaneous movements to cause, in the significantly larger percentage of *S*s, a consistent precedence of the less-used finger. In consequence, motor leads, whether movements of the fingers or action-currents, cannot be considered an index of 'native handedness.' The indirect influence of learned preferences for hand-use is a factor which raises serious doubts about the value of existing tests intended to indicate a 'native laterality.'

The following specific results form the basis for these conclusions:

(1) The less-used finger consistently moved first in 66.7% of the comparisons. The opposite was true in 15.8%, while in 17.5% of the cases neither finger led consistently.

(2) The greater the disparity in use between the fingers involved, the more pronounced was the tendency for over-compensation in time relationships. The little finger of the non-preferred hand consistently led the forefinger of the preferred hand in 87.5% of the cases. The forefinger led in 7.5%, and 5% of the cases were indeterminate.

(3) The tendency for one finger to precede the other was constant for

[27] J. M. Smith, The sensory function of the non-preferred hand, *J. Exper. Psychol.*, 16, 1933, 271-282.

[28] J. M. Smith, Which hand is the eye of the blind? *Genet. Psychol. Monog.*, 5, 1929, 248.

the individual both in terms of finger indicated and in proportion of leads. A correlation between the percentages derived from 30 repetitions of the experiment was $+0.658 \pm .066$.

(4) The effect of fatigue was to cause a consistent precedence of the movement of the fatigued over the non-fatigued finger.

(5) The weight lifted by the less-used finger was consistently judged heavier in measures of weight discrimination. This was true in 80% of the cases.

(6) The greater the disparity between the fingers the greater was the tendency to overestimate the weight on the less-used finger. The weight lifted by the little finger of the non-preferred hand was judged heavier than the weight on the forefinger of the preferred hand in 90% of the cases.

(7) The correlation between the relative weight estimation by the fingers and the proportion of leads in 'simultaneous' movement was $+0.524 \pm .045$.

(8) No consistent difference was found between the fingers in the amount of pressure exerted in attempted equal pressure. There was as great a tendency for the less-used finger to press harder (39.2%) as for the more-used (43.3%).

(9) In all cases of greater pressure by the less-used finger there was a greater average tendency for over-compensation in relative priority of movements. There was an increase of 3.7% in the average proportion of leads by the less-used fingers.

(10) A relatively smaller pressure by the less-used finger is, on the average, indicative of a greater estimation of the weights lifted by that finger. The difference for the total group of 3.6 gm. probably means that the greater estimation of the pressure prevented equal or greater actual pressure.

STUDIES OF MORPHINE, CODEINE, AND THEIR DERIVATIVES

V. The Use of Maze-Trained Rats to Study the Effect on the Central Nervous System of Morphine and Related Substances

By A. Kenneth Simon and Nathan B. Eddy, University of Michigan

In connection with the systematic study of morphine and related substances a search was instituted for a method which would provide a quantitative measure of the action of such substances on the central nervous system in an intact animal without the use of an anaesthetic or mechanical restraint.

In 1920, Macht and Mora[1] compared the effects of the opium alkaloids on the performance of rats in a circular maze and were able to show marked interference with activity. More recently Miles[2] has recommended the use of rats trained on an elevated maze for the demonstration of qualitative differences in the action of drugs on the central nervous system, and Lashley[3] in his decortication experiments has proved involvement of the cortex in maze-learning and retention. Regularity of performance of normal maze-trained rats has been recorded repeatedly by various observers so that in so far as this regularity is related to integrity of the cerebral cortex deviations in the regular performance of trained rats under the influence of morphine and allied substances may be related to an effect on the cerebral activity.

In the present paper a method will be discussed which measures quantitatively the normal behavior of maze-trained rats and modifications thereof under the influence of morphine. Subsequently the application of the method to a comparison of the effects of substances derived from morphine will be described.

Because of the ease of observation an elevated maze of the type described by Miles has been employed exclusively. This consisted of a number of sections of supported runway, each section providing a running surface 1 in. wide and 3 ft. long.

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[1] D. I. Macht and C. F. Mora, Effect of opium alkaloids on the behavior of rats in the circular maze, *J. Pharm. & Exper. Therap.*, 16, 1920, 219-235.

[2] W. R. Miles, Drug effects measured by acquired patterns of response, *Amer. J. Physiol.*, 90, 1929, 451.

[3] K. S. Lashley, *Brain Mechanisms and Intelligence*, 1929, 47.

These sections were arranged in continuation with and at right angles to one another so that a pattern was established, as illustrated in Fig. 1, consisting of a pathway to food, the true-path, from which blind ends projected. The final section had a small metal food-cup fastened at F, which will hereafter be referred to as *food*. The cup measured about ¾-in. in each dimension and was placed slightly below the surface of the maze so that it was not readily visible. The true-path was the direct route to food; the deviations were the blind ends. The surface of the maze runway was 30 in. from the floor and in the pattern illustrated the true path measured 28.5 ft.

Both albino and hooded rats have been used, the latter derived by crossing albino and Norwegian black rats. They were segregated according to sex and kept in metal cages, 10-12 rats to the cage. The bottom of the cage was made of ½-in. mesh wire so that excreta fell at once to the tray underneath. No food was placed in the storage-

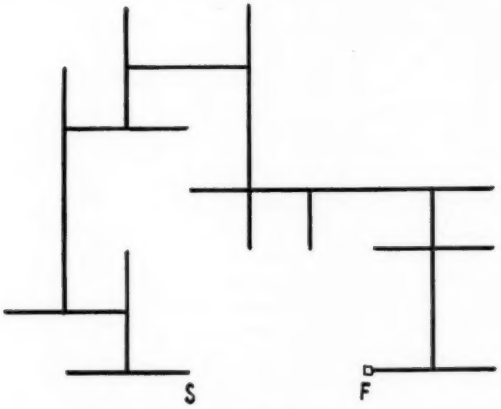

FIG. 1. PATHWAY OF ELEVATED MAZE

cage but plenty of fresh water was kept available in a drinking bottle. The rats were run on the maze in the afternoon and fed individually at the completion of their run. No species or sex differences have been observed, except that male rats seem to overcome their initial shyness more readily than females and consequently are a little more easily trained to the maze.

The rats were fed a mixture made up as follows: 1 can (10½ oz. by wt.) tomato soup, 1 can (10½ oz. by wt.) pea soup, 250 cc. water, 10 cc. cod liver oil, 200 gm. rolled oats, 300 gm. whole wheat flour, 650 gm. coarse yellow cornmeal. If necessary a little more cornmeal was added to make a very stiff mush. Each rat was fed about 20 gm. daily of the food-mixture, the amount per rat varying somewhat according to the size and avidity of each animal. The quantity was that which the rat would clean up quickly, in about 15-20 min., and which would enable the rat to hold its weight or gain slightly. Further reference to the feeding will be made later.

Before being trained to the maze, growing and stock rats were fed a mixture

consisting of whole wheat flour 200 parts, powdered milk 100 parts, sodium chloride 3 parts. This food was available to them at all times in their cage.

Training of the rats to the maze was begun by selecting about a dozen rats not previously used for any purpose, of the same sex and of approximately equal size (preferably about 150 gm.) and placing them in a fresh cage without food. Twenty-four hours later the group were placed together on the maze and left to themselves for a half-hour except for placing back on the maze those which attempted to crawl or jump to the floor. Then each rat was placed in an individual compartment and given 20 gm. of food. At the end of another half hour the rats were returned to their cage. This procedure was repeated daily at the same time, the feeding time being shortened gradually to 20 min. Most rats became accustomed to the new regime of handling and feeding in about a week although occasionally excessively shy rats required a little longer adjustment-period. During this period as the rats overcame their shyness they began to explore the maze. When the rats were considered to be satisfactorily adjusted they were put on the maze individually and taught the true path to food. This required, as a rule, an additional week or 10 days. The teaching was facilitated by showing the rat the food at F and then placing the animal back on the maze, increasing distances as it learned the path. Practically all rats will learn the maze, though their time for learning will vary considerably and occasionally a rat will show erratic behavior over a prolonged period. In our experience, therefore, about a tenth of the rats introduced to the maze have been discarded as a more economical procedure than prolonged training.

When the group of rats knew the maze; that is, when they would start promptly when placed on the first section and run rapidly along the true path to food with rare deviations, their behavior was timed. From then on each rat was given each day at the same time three runs on the maze followed by feeding and the timing was repeated once a week. In the sixth week, on the day following the timing, one hour before the usual running time the rats were injected subcutaneously with morphine or other substance to be tested. One hour later each rat was given its three runs, its behavior was timed, and it was fed as usual. The following day also its behavior was timed. After this the rats were injected at intervals of not less than one week. At the time of each injection the same procedure was followed—the rat's behavior was timed the day before, the day of, and the day following the injection. The rat ran at the same time each day and the injection was given one hour before the regular running-time. The same animals should not be used longer than three months after the first injection.

The procedure just described is that adopted after two years' experience. Some of the steps leading to its adoption will be discussed later.

The following records were made of the rats' behavior; where time was taken it was measured by two stop-watches:

(1) 'Initial delay' = the time from the placing of the rat on the maze to its passing of the first turn; that is, until it is definitely started on the path to food.

(2) 'Active time' = the time spent in forward progression from the first to the last turns of the maze.

(3) 'Total time' = the time from the placing of the rat on the maze to its arrival at food.

(4) 'Waste time' = the difference between total time and the sum of initial delay and active time, the time consumed by stops and errors.

(5) 'Stops and errors' = the number of stops along that portion of the maze over which 'active time' is measured, and the number and location of errors are noted. Stops

and errors may be due to uncertainty, to distractions, or to inattentiveness to the main purpose, the finding of food.

(6) Animal's weight and amount of food consumed.

(7) Incidental observations such as disturbances of gait, variations in reflexes, and muscle tone.

The behavior of a rat on the starting section and the time it delays before starting are characteristic for the animal. During the preliminary training-period the initial delay is usually longer than subsequently, but in trained animals kept under controlled conditions, it is remarkably constant. There is also an almost constant tendency for the initial delay of a particular rat on the second and third runs of a particular day to be shorter than the initial delay on the first run. Animals which show excessively long or very irregular initial delays are usually extremely shy, have not become satisfactorily adjusted to the feeding regime, or are being under- or overfed. Such animals were not injected until their initial delays shortened and became regular. Little difficulty was experienced in this respect because of the adoption of the 6-weeks training-period.

In rats under the acute influence of morphine the initial delay is lengthened. This lengthening is usually associated with a decrease in spontaneous activity, a decrease in responsiveness to various external stimuli unless suddenly applied and a marked tendency to an invariability of response. These modifications in behavior indicate a decreased awareness of the surrounding conditions and may be explained first of all by central cerebral depression. At the same time the recent work of Stender and Amsler suggests,[4] though it does not conclusively prove, that morphine depresses sensory nerve-endings. Such an effect also, since it would cause an animal to become isolated to a greater or less extent from its environment, would be expected to affect the interval elapsing between the moment in which an animal is placed in a situation (placed on the starting section) and the moment in which it indicates its awareness of that situation (starting for food). In the further elaboration of this possibility it must be admitted that any interference with afferent, efferent, or intercollated pathways would probably similarly exert an influence on the duration of the initial delay.

Hunger is the primary stimulus to rapid successful performance of the rats in the method described and only animals suffering the discomfort of hunger have low regular initial delays. Since morphine is known to affect the smooth muscle of the stomach it might by this local action limit the setting up of hunger in the animal and thus lengthen initial delay. Injected rats whose initial delays have been lengthened eat food when it is given them but often show less desire for food and start and finish their meal less rapidly than normal rats. It should be pointed out that this might

[4] O. Stender and C. Amsler, Beiträge zur Kenntnis der Morphengewöhnung, *Arch. f. exper. Pathol. u. Pharm.*, 160, 1931, 195-205.

be due to central depression interfering with or suppressing appetite and the perception of hunger fully as well as it might be due to a local effect on the stomach. Carlson says that if food is placed directly into the stomach at the height of a period of gastric hunger contractions the latter are in-

TABLE I

MAZE-BEHAVIOR OF 20 RATS; FIRST RUN MADE 10 MIN. AFTER ONE-TENTH OF USUAL DAILY RATION

Date (1933)	Av. per rat per run			Av. per rat per day	
	Initial delay	Active time	Waste time	Stops	Errors
April 25	33.4	10.7	28.8	10.3	2.3
" 26	31.2	10.6	27.8	9.8	1.7
" 27	25.8	11.0	36.3	8.8	1.5
" 28	30.0	10.5	32.7	10.6	1.7
May 2	34.2	11.0	31.9	12.4	1.4
" 3	35.1	10.5	35.0	11.4	1.6
" 4	28.7	10.7	30.9	11.4	1.7
" 5	22.3	10.5	27.7	8.4	1.7
" 8	21.1	10.3	21.3	10.4	1.0
" 10	31.4	11.3	28.6	7.9	1.0
" 11	26.3	11.3	25.5	8.7	1.9
" 12	34.2	11.6	39.9	12.0	2.0
Av.	29.4	10.9	30.5	10.2	1.6

TABLE II

MAZE-BEHAVIOR OF 20 RATS OF TABLE I, WITHOUT PRELIMINARY FEEDING

Date (1933)	Av. per rat per run			Av. per rat per day	
	Initial delay	Active time	Waste time	Stops	Errors
March 22	14.3	11.5	8.5	5.5	0.4
" 29	13.5	10.1	6.5	4.1	0.1
April 18	17.1	11.1	8.7	5.5	0.4
" 19	24.8	11.3	17.7	9.2	1.0
" 20	21.1	10.1	10.0	8.4	0.5
" 21	17.0	10.3	11.0	6.8	0.2
May 16	20.4	10.6	24.7	9.0	1.2
" 23	16.1	11.8	23.1	11.5	1.0
" 28	14.0	9.7	8.8	6.3	0.5
June 5	16.0	10.3	14.3	7.2	0.5
" 13	23.3	10.8	25.2	6.1	0.4
" 21	15.2	10.2	11.7	7.3	0.1
Av.	17.7	10.7	14.2	7.3	0.5

hibited at once.[5] Feeding a small portion of food, therefore, might be expected to suppress the local factor in the setting up of hunger and leave intact appetite and the central portion of the hunger mechanism. Accordingly, two groups of 10 rats each were fed one-tenth of their regular

[5] A. J. Carlson, *The Control of Hunger in Health and Disease*, 1916, 99.

meal and then their performance was timed in the regular way, the timing being done ten minutes after the feeding.

These observations are summarized in Table I and an equal number of control observations on the same rats in Table II. Comparison of the figures in these two tables shows that the preliminary feeding did increase initial delay, though, as a rule, not very markedly. It increased 'waste time' decidedly and increased the number of both stops and errors, but it had no

TABLE III

EFFECT OF SUBCUTANEOUS ADMINISTRATION OF MORPHINE HYDROCHLORIDE; FIRST INJECTION AFTER 5–6 WEEKS OF REGULAR MAZE-RUNNING

No. of rats	Day	Dose (mgm. per kilo.)	Av. per run			Total per day				No. rats affected
			Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	12.4	13.7	9.1	38	4	0	0	—
12	Injection	3	30.4+	15.0*	12.2*	62	0	6	0	4
	Following	—	9.4	13.9	10.4*	53	2	0	2	—
	Previous	—	13.1	13.3	6.3	81	7	0	0	—
12	Injection	4	30.6+	15.6*	30.2*	101	5	4	1	4
	Following	—	7.5	13.9	5.4	38	0	0	0	—
	Previous	—	5.0	10.2	1.5	4	0	0	0	—
10	Injection	5	19.6	15.0*	8.0*	51	2	0	1	5
	Following	—	5.3	10.1	1.4	1	0	0	0	—
	Previous	—	9.8	9.0	2.9	17	2	0	0	—
9	Injection	6	57.6+	11.5*	39.0*	27	7	6	5	9
	Following	—	12.8	10.7	6.4	64	1	0	0	—
	Previous	—	12.9	10.3	14.1	57	12	0	0	—
10	Injection	7	50.9+	12.5*	12.6*	46	8	9	1	6
	Following	—	19.1	10.6	24.2	104	10	0	0	—
	Previous	—	8.8	9.9	4.9	29	3	0	0	—
10	Injection	10	89.3+	9.5*	4.3*	19	1	19	3	8
	Following	—	9.0	9.4	4.6	32	2	0	0	—

* Figures are for runs completed

definite effect on 'active time.' Now comparing these results with the effect of morphine (Table III), the drug caused a much greater increase in 'initial delay.' It also caused an increase in stops and 'waste time' and a definite increase in 'active time.' Therefore, even if a local effect of morphine suppresses hunger-contractions and thus contributes to the lengthening of 'initial delay,' some additional factor would seem to be involved.

Besides possible central and peripheral effects already considered other changes which might affect 'initial delay' are respiratory and circulatory slowing and a direct effect upon the skeletal muscles. These last factors,

however, should interfere primarily with activity in general and, unless they affect the '*initial delay*' to a greater extent than they affect the ability of the animal to run, their rôle in the lengthening of 'initial delay' should be apparent from the increase in the 'active time.'

From this listing and discussion of the factors which may increase 'initial delay' it is concluded that this time-interval, or rather its modification under the influence of morphine, may be looked upon as an expression of the integrated effect of morphine on the behavior of the animal and a function of the action of morphine on the various systems, with an effect on the cerebrum and the rest of the nervous system probably playing the major rôle.

Normal trained rats usually have short 'waste times,' while under the influence of morphine the 'waste time' is sometimes increased and sometimes not. When it is affected, however, it is, in most cases, very considerably affected. Though a morphinized rat often runs the maze without a stop, when it does stop it tends to remain inactive for a long time, just as it tends to remain inactive on the starting section.

Stops, the principal components of 'waste time,' may be an expression of very different effects. The normal rat will stop and give attention to anything strange and unfamiliar. These stops will be brief usually, they may only be for a tenth of a second, and the more constant controllable conditions in the laboratory are kept the less frequent will be their occurrence. Nevertheless, when they do occur they appear to be an indication of awareness of the surroundings, and one would expect a decrease in these stops in animals whose cerebrum is depressed.

There are other situations in which stops appear to be related rather to uncertainty and confusion than to intelligence. This appears in rats learning the maze and occasionally during the performance of morphinized rats. In these cases stops are often at the cross-roads, where the animal seems uncertain of the direction to take, and are often associated with retraces and slow forward movement. These stops one would expect to be increased in animals whose cerebrum is depressed.

Errors decrease very quickly in number as fresh rats learn the maze. Thereafter retraces are rare and errors occasional. They are not increased significantly under the acute influence of morphine; the activity of the animal is affected sooner than its ability to run the maze correctly.

The ability of the injected rats to run an open maze without error does not eliminate the possibility of cerebral depression but only indicates that the critical mass of functioning cortex necessary for this activity is unaffected. Lashley's work indicates that the cortex is essential to open-maze retention, but also that more cortex must be destroyed before open-maze retention is interfered with than it is necessary to destroy in order to interfere with closed-maze retention. Maier has shown that solution of the problem-maze is dependent upon a greater amount of functioning cortex than is maze-retention or learning.[6] Experiments with the problem-maze conducted in this laboratory have demonstrated that small doses of morphine affect the rats' 'initial delay' before inability to solve the maze can be demonstrated, the inertia

[6] N. R. F. Maier, The effect of cerebral destruction on reasoning and learning in rats, *J. Comp. Neurol.*, 54, 1932, 45-75.

in rats which retain their ability to solve the problem-maze increasing until they fail to leave the starting table.

This analysis then of the various phases of the rat's behavior on the elevated maze indicates that the 'initial delay' would be the best quantitative index for comparison of the action of morphine and its derivatives. Accordingly, the average of the initial delays for the 3 runs on the day of injection in comparison with the average of the initial delays of the 3 runs on the previous day has been selected as the criterion of the presence or absence of effect of the drug. An animal is considered to be affected—'depressed'—when the injection-day average is at least double that of the previous day and at the same time exceeds the latter by at least 15 sec. This quantitative minimum for the determination of the presence of effect has been arrived at from a close study of the variation in normal behavior of very many rats on many successive days. The minimal effective dose of the drug is defined as the smallest dose which affects to the extent just described the majority of a group of eight or more rats. On account of individual variations in the responsiveness of different rats to morphine and allied substances, larger groups are, however, advisable and were almost invariably employed in determining the effect of any specific dose.

In order not to prolong the procedure unduly if an injected rat remained on the starting section of the maze for two minutes it was lifted and that particular run was recorded as a "no start" (N.S.). Under such circumstances the animal's initial delay would be 120+ sec. and obviously no other information or timing of the rat's behavior on the maze is obtainable for that run. Rarely a rat would start but would go only part way along the maze and then become quiet or return to the starting section and at the end of several minutes would not have reached food. In such a case the rat was also lifted and its behavior recorded as an "incomplete run" (I.R.). Under such circumstances a figure is available for initial delay and a plus figure for waste time but active time and total time are not recorded. Table III summarizes in the terms of this description the effect of different doses of morphine when the dose is the first given to a trained rat.

At the beginning of this study the training and injection procedure differed in two particulars from that which has just been described. Both of these modifications are important and have been the subject of prolonged study. They have to do with the mode of administration of the drug and the prolonged training period before any drug is given.

Primarily all injections were given intraperitoneally and the rats were injected as soon as possible after they had learned the maze, usually a week to 10 days after the rat began to run the maze successfully. Table IV shows the effect on each of 10 rats of 10 mgm. per kgm. of morphine hydrochloride injected in this way. The average initial delay for at least

5 of the rats and for the whole group is definitely lengthened and 11 of the 30 trials for the 10 rats were recorded as 'no starts.' About four and a

TABLE IV

EFFECT ON BEHAVIOR OF MORPHINE HYDROCHLORIDE INJECTED INTRAPERITONEALLY AFTER RATS HAD LEARNED THE MAZE ON AUGUST 13, 1931

Rat no.	Date (1931)	Dose (mgm. per kilo.)	Av. per run			Total per day			
			Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs
	Aug. 27	—	7.3	8.9	1.4	1	0	0	0
1	" 28	10	33.3	11.9	4.1	0	0	0	0
	" 29	—	4.3	8.2	0.8	0	0	0	0
	" 27	—	2.6	8.4	6.3	1	1	0	0
2	" 28	10	120.0+	—	—	—	—	3	—
	" 29	—	4.3	9.8	4.5	4	2	0	0
	" 27	—	1.6	6.5	0.9	2	0	0	0
3	" 28	10	120.0+	—	—	—	—	3	—
	" 29	—	1.0	7.4	0.5	0	0	0	0
	" 27	—	4.5	10.3	5.5	1	1	0	0
8	" 28	10	5.2	10.5	1.5	0	0	0	0
	" 29	—	13.0	10.5	2.7	3	0	0	0
	" 27	—	3.6	8.2	1.2	0	0	0	0
10	" 28	10	2.4	8.6	2.0	0	0	0	0
	" 29	—	7.5	9.2	1.6	1	0	0	0
	" 31	—	3.7	8.5	4.1	0	2	0	0
6	Sept. 1	10	83.0+	12.0*	13.0*	2	1	2	0
	" 2	—	6.0	8.7	4.0	1	2	0	0
	Aug. 31	—	7.6	10.2	1.8	0	0	0	0
12	Sept. 1	10	14.6	11.6	5.4	4	0	0	0
	" 2	—	8.0	12.0	10.2	0	3	0	0
	Aug. 31	—	3.3	8.3	0.5	2	0	0	0
16	Sept. 1	10	11.3	10.2	5.0	8	0	0	0
	" 2	—	6.0	9.6	5.4	6	0	0	0
	Aug. 31	—	4.3	7.4	3.3	3	0	0	0
102	Sept. 1	10	5.2	8.1	2.2	4	0	0	0
	" 2	—	5.0	6.7	7.2	0	3	0	0
	Aug. 31	—	6.0	7.5	1.8	0	0	0	0
108	Sept. 1	10	120.0+	—	—	—	—	3	—
	" 2	—	7.6	8.2	4.1	2	1	0	0

Av. initial delay per rat per run: previous day, 4.5; injection day, 51.5+ (5 rats affected); following day, 6.2.

* Figures are for run completed.

half months later 11 and 12 mgm. per kgm. of morphine hydrochloride had very little effect on the initial delay in another group of ten rats which

TABLE V

EFFECT OF MORPHINE HYDROCHLORIDE INJECTED INTRAPERITONEALLY SEVERAL MONTHS AFTER RATS HAD LEARNED THE MAZE

(These rats began to run the maze at the same time as those in Table IV. Rats 3, 12, and 102 are common to both groups.)

Rat no.	Date (1932)	Dose (mgm. per kilo.)	Av. per run			Total per day			
			Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs
	Jan. 11	—	5.0	14.9	0.9	1	0	0	0
101	" 12	11	8.3	14.4	1.9	1	0	0	0
	" 13	—	7.0	15.1	6.4	2	1	0	0
	" 11	—	4.3	14.8	0.2	0	0	0	0
102	" 12	11	5.0	14.9	1.1	0	0	0	0
	" 13	—	7.6	16.2	2.8	2	0	0	0
	" 11	—	7.6	14.3	0.7	1	0	0	0
103	" 12	11	46.6	15.6	1.6	0	0	0	0
	" 13	—	14.3	13.0	1.6	1	0	0	0
	" 11	—	7.0	17.2	0.4	1	0	0	0
107	" 12	11	13.0	18.9	0.9	0	0	0	0
	" 13	—	12.3	18.8	7.4	6	0	0	0
	" 11	—	7.0	16.0	0.2	0	0	0	0
112	" 12	11	25.0	16.9	0.7	0	0	0	0
	" 13	—	10.6	15.9	1.2	0	1	0	0
	" 12	—	2.6	17.3	0.7	0	0	0	0
3	" 13	12	6.6	23.9	5.5	2	1	0	0
	" 14	—	4.3	17.9	1.1	2	0	0	0
	" 12	—	3.0	15.3	1.9	1	0	0	0
4	" 13	12	7.6	17.9	3.3	3	0	0	0
	" 14	—	3.0	13.9	0.9	0	0	0	0
	" 12	—	1.3	14.2	0.7	0	0	0	0
5	" 13	12	3.3	15.1	1.2	0	0	0	0
	" 14	—	2.6	15.3	2.9	0	1	0	0
	" 12	—	3.2	16.2	1.2	0	0	0	0
12	" 13	12	5.6	17.5	1.7	0	0	0	0
	" 14	—	3.6	15.6	0.2	0	0	0	0
	" 12	—	6.0	15.4	2.4	2	0	0	0
19	" 13	12	22.0	15.4	5.1	0	1	0	0
	" 14	—	5.3	14.6	0.8	0	0	0	0

Av. initial delay per rat per run: previous day, 4.7; injected day, 14.3 (3 rats affected); following day, 7.0.

had been running the maze daily through all of this time and had received weekly morphine doses of varying size (see Table V).

Only three rats were slightly affected and the average initial delay for the whole group shows little increase, less than the minimum criterion which has been adopted

for determining the presence of an effect. For some reason the susceptibility of the rats to the effect of morphine has been decreased. Indeed, at this time the dose of morphine had to be increased to 30 mgm. per kgm. before an effect at the end of an hour comparable to that shown originally by 10 mgm. per kgm. (Table IV) was obtained. This change in susceptibility is not uniformly progressive. Most of the change takes place during the first month and by far the greater part between the first and second weeks, that is, between the first and second doses. After the first month, the decrease in susceptibility is very slight and often not at all apparent from week to week, a relative plateau being reached in about 4-6 weeks after the

TABLE VI

EFFECT ON INITIAL DELAY OF REPEATED INTRAPERITONEAL DOSES OF MORPHINE HYDROCHLORIDE IN RATS WHICH HAD RUN THE MAZE MORE THAN FOUR MONTHS, RECEIVING DURING THAT TIME REPEATED DOSES OF MORPHINE AND CODEINE

Date (1932)	Dose (mgm. per kilo.)	Av. initial delay per run						No. of 'no-starts'
		Rat no.					Av. for group	
		113	114	116	117	118		
Jan. 13	—	6.0	7.3	3.0	7.3	6.0	5.9	0
" 14	13	32.6	12.6	37.3	22.3	17.3	24.3	0
" 20	—	3.3	3.0	5.3	6.3	3.3	7.1	0
" 21	20	120.0+	11.6	8.0	12.3	22.0	34.8	3
" 27	—	4.3	3.6	5.6	6.3	3.3	7.7	0
" 28	30	120.0+	51.0	16.3	22.0	120.0+	65.8+	6
Feb. 3	—	6.3	3.6	17.6	5.0	11.3	8.7	0
" 4	30	120.0+	16.3	50.6+	28.6	120.0+	67.1+	7
" 10	—	3.6	3.0	3.6	5.0	3.6	3.7	0
" 11	30	120.0+	8.6	70.0	78.6+	106.6+	76.7+	6
" 18	—	4.3	2.0	1.3	3.3	1.6	2.5	0
" 19	30	120.0+	12.3	73.0+	90.6+	120.0+	83.1+	8
" 25	—	4.3	2.3	3.0	4.3	3.0	3.3	0
" 26	30	120.0+	21.6	44.6+	10.3	120.0+	63.3+	7
Mar. 3	—	5.6	3.3	2.0	3.6	1.6	3.2	0
" 4	13	54.0+	12.3	17.0	12.3	9.3	21.0+	1
" 10	—	4.0	2.6	2.6	4.0	1.6	2.9	0
" 11	13	36.0	13.6	8.6	9.3	8.0	15.1	0

rat has learned the maze. Table VI shows the effect of repeated doses of morphine on the same rats after the relative plateau had been reached. These rats were trained to the maze early in September, 1931, and were injected at intervals during September, October, November, and December with morphine and codeine.

Among the possible explanations of this phenomenon are: (1) change in absorbability or handling of the drug because of repeated intraperitoneal injection; (2) development of tolerance; and (3) some change associated with repeated maze-running and more perfect learning of the maze, *e.g.* the development of maze-running as a conditioned reflex, or such a change in the nervous mechanism controlling maze-running that the depression of morphine must spread through a greater mass of the central nervous

TABLE VII

EFFECT ON INITIAL DELAY OF REPEATED ADMINISTRATION OF MORPHINE

(Rats ran maze continuously from April 13, 1932, and were injected only at the end of the first, fifth, ninth, tenth, eleventh, twelfth and thirteenth weeks.)

Date (1932)	Dose (mgm. per kilo.)	Mode of administration	Av. initial delay per rat per run										Av. for group	No. of 'no-starts'
			35	36	37	38	39	40	41	42	123	132		
April 18	—	—	5.0	4.3	5.0	4.6	6.6	8.6	8.0	18.3	5.0	11.6	7.7	0
" 19	13	Intraperitoneal	120.0+	120.0+	120.0+	21.6	120.0+	82.6+	120.0+	120.0+	53.0	105.6+	105.9+	20
May 16	—	—	6.3	4.6	4.6	8.0	6.6	8.0	7.3	9.3	11.0	19.0	8.4	0
" 17	13	Intraperitoneal	108.0+	37.6	24.3	92.3+	120.0+	119.0+	42.6	118.6+	41.6	103.3+	80.7+	10
June 12	—	—	4.3	2.3	3.3	7.0	8.3	6.6	10.6	9.0	6.3	7.6	6.5	0
" 13	13	Intraperitoneal	35.6	5.6	10.0	52.6	120.0+	68.6	19.3	47.3	59.3	49.0	46.7+	3
" 21	—	—	31.6	11.6	12.6	21.0	38.3	17.3	18.6	16.3	29.6	8.3	20.5	0
" 22	13	Subcutaneous	120.0+	28.6	20.6	44.6	120.0+	120.0+	82.3+	120.0+	120.0+	120.0+	89.6+	19
" 28	—	—	9.6	22.6	14.0	29.0	24.0	9.3	11.0	15.6	9.3	5.3	14.9	0
" 29	13	Subcutaneous	66.6	28.3	58.3	51.6	86.6+	30.0	96.0+	36.6	50.6	46.0	55.0+	4
July 5	—	—	13.0	18.6	5.6	34.6	56.6	24.6	18.6	25.3	20.6	5.0	22.2	0
" 6	13	Intraperitoneal	46.3	24.6	8.6	34.0	90.0+	34.0	34.0	39.0	26.6	13.3	35.0+	1
" 12	—	—	19.6	25.6	22.0	43.3	58.3	14.0	35.3	36.3	11.3	5.0	27.0	0
" 13	13	Subcutaneous	120.0+	66.6	90.0+	120.0+	104.0+	120.0+	120.0+	91.6+	37.0	43.3	91.2+	16

TABLE VIII

EFFECT ON INITIAL DELAY OF REPEATED ADMINISTRATION OF MORPHINE

(Rats injected weekly beginning one week after they had learned the maze: off the maze during the second, third, fourth, sixth, seventh, and eighth weeks.)

Date (1932)		Dose (mgm. per kilo.)	Mode of administration	Av. initial delay per rat per run										Av. for group	No. of 'no-starts'
				20	22	28	29	30	31	34	119	120	121		
April	4	—	—	9.0	6.0	2.6	4.0	5.6	12.3	7.0	5.0	4.0	6.0	6.1	0
"	5	13	Intraperitoneal	120.0+	46.6+	23.0	26.0	28.3	120.0+	30.0	120.0+	6.0	31.3	55.1+	10
May	2	—	—	4.0	3.3	1.0	2.0	3.3	9.6	8.3	7.6	5.3	7.0	5.1	0
"	3	13	Intraperitoneal	16.0	7.3	5.6	1.6	91.0+	101.0+	13.0	54.3	62.3	90.0+	44.2+	6
"	30	—	—	5.0	3.0	2.0	1.3	4.0	10.0	11.3	10.0	7.6	4.6	5.8	0
"	31	13	Intraperitoneal	6.0	4.3	1.3	3.6	81.0+	29.3	119.0+	22.1	29.3	13.6	30.9+	4
June	6	—	—	5.3	2.6	1.0	1.6	3.0	10.3	9.3	17.0	5.0	8.0	6.3	0
"	7	13	Subcutaneous	4.3	61.3	6.0	4.3	6.3	60.0+	60.0+	120.0+	38.3	120.0+	48.0+	8
"	12	—	—	5.0	3.0	1.6	2.0	4.3	11.0	7.6	5.3	4.6	5.3	4.9	0
"	13	13	Intraperitoneal	7.0	37.6	9.0	5.3	120.0+	37.3	38.0	39.3	18.0	20.3	33.1+	3
"	20	—	—	7.0	7.6	9.0	4.0	14.3	30.3	2.3	27.6	30.3	68.6	20.1	0
"	21	13	Subcutaneous	14.0	58.3+	8.6	5.6	120.0+	90.0+	66.3	120.0+	47.3	120.0+	65.0+	11
"	27	—	—	5.0	4.0	2.5	2.8	4.3	19.4	5.3	12.7	7.0	8.9	7.1	0
"	28	13	Subcutaneous	20.0	15.6	3.5	20.0	120.0+	120.0+	48.3	120.0+	54.6	80.0	60.2+	9

TABLE IX

EFFECT ON INITIAL DELAY OF REPEATED SUBCUTANEOUS DOSES OF MORPHINE HYDROCHLORIDE

(Rats began to run the maze regularly ten days before first injection, ran daily thereafter, and were injected once a week.)

Date (1932)	Dose (mgm. per kilo.)	Av. initial delay per rat per run									Av. for group	No. of 'no-starts'
		311	312	313	315	316	317	318	319	320		
Aug. 14	—	11.6	3.6	4.6	4.6	2.6	4.6	4.3	5.6	3.3	4.9	0
" 15	10	110.0+	46.6+	32.3	36.3	50.0	32.0	96.6+	12.3	58.3+	52.7+	5
" 21	—	15.0	2.0	2.6	4.0	18.0	7.6	4.6	2.3	5.3	6.8	0
" 22	10	120.0+	4.3	41.6	4.6	120.0+	30.0	25.6	5.6	7.0	39.8+	5
" 28	—	10.3	10.6	5.3	1.6	11.3	14.0	12.0	5.6	8.3	8.5	0
" 29	10	28.3	55.0	120.0+	23.0	40.3	53.3	60.3	6.0	15.3	44.6	4
Sept. 5	—	22.6	3.0	6.0	5.3	12.0	4.3	11.6	9.6	8.3	9.2	0
" 6	10	29.3	6.0	120.0+	23.3	44.3	16.0	20.0	59.3	47.3	40.6	3
" 12	—	30.3	5.6	24.0	8.0	11.6	6.0	12.0	26.0	7.3	14.5	0
" 13	10	40.3	15.3	50.6	22.6	46.0	26.6	24.6	16.6	10.6	28.1	0
Oct. 10	—	25.3	8.3	16.6	8.0	17.6	13.3	15.6	9.6	13.3	14.2	0
" 11	10	38.0	12.0	35.3	11.6	76.0	61.0	20.0	46.0	21.0	35.6	0

TABLE X

EFFECT ON INITIAL DELAY OF REPEATED SUBCUTANEOUS DOSES OF MORPHINE HYDROCHLORIDE

(Rats had been running the maze regularly for a month before first injection, ran it daily thereafter, and were injected at the times indicated.)

Date (1932)	Dose (mgm. per kilo.)	Av. initial delay per rat per run									Av. for group	No. incomplete runs	No. of 'no-starts'
		301	302	303	304	305	306	307	309	310			
Sept. 1	—	13.6	5.3	6.0	5.0	7.3	6.0	9.0	2.3	4.0	6.5	0	0
" 2	10	120.0+	120.0+	95.0+	63.0	86.6	120.0+	120.0+	83.0+	53.0+	95.5+	1	16
" 29	—	42.3	60.6	12.0	6.0	35.0	44.6	47.6	5.3	9.6	29.2	0	0
" 30	10	120.0+	120.0+	120.0+	120.0+	66.0	120.0+	103.3+	57.3	23.0	94.4+	0	16
Oct. 6	—	33.3	55.3	16.0	12.3	35.0	42.0	32.6	7.6	12.0	27.5	0	0
" 7	10	120.0+	120.0+	117.3+	35.3	51.0	118.0+	69.0+	31.0	120.0+	86.8+	1	14
" 13	—	44.0	26.6	9.6	13.6	19.6	31.0	41.0	9.6	11.0	22.9	0	0
" 14	10	120.0+	94.3+	117.6+	20.6	58.0	119.3+	120.0+	78.6	37.0	85.0+	2	13
" 27	—	29.0	48.0	10.3	8.6	22.6	23.0	54.0	15.0	34.6	27.2	0	0
" 28	10	120.0+	84.6+	51.0	16.6	42.3	113.6+	120.0+	113.3+	57.3	73.5+	1	10

system before an effect on the behavior of the rat on the maze can be demonstrated.

In connection with the first possibility, when the phenomenon of decreased susceptibility had been demonstrated in two groups of 10 rats each (being run for another purpose which will be described presently), the weekly dose of morphine hydrochloride was given subcutaneously instead of intraperitoneally. The result is shown in Tables VII and VIII. Undoubtedly under these circumstances at the end of an hour morphine was more effective in lengthening initial delay when given subcutaneously than when given intraperitoneally. It is possible that this difference is due to slower absorption from the subcutaneous tissues, so that the 1-hr. observation more closely approaches the peak of effect after this mode of administration and that there has been a change in intraperitoneal absorption toward an increase or speeding up instead of the reverse. In any case, if the first injection given to fresh rats shortly after they have learned the maze is subcutaneous and the same dose is repeated weekly by this channel of administration the same phenomenon of early and rapid decrease in susceptibility to a plateau of effect develops. (Table IX). On the other hand, the phenomenon is very nearly eliminated if the rats are allowed to run the maze regularly for at least a month before the first injection is given (Table X). As with intraperitoneal injections, a very slight decrease in susceptibility may develop even under these conditions but does not progress sufficiently in three months to affect one's judgment of the number of animals affected or the minimal effective dose (compare Tables III and XII).

Although we have no knowledge of any evidence suggesting that tolerance to morphine can develop when small doses are given not more often than once a week, but have rather much evidence that such is not the case, a specific experiment was devised to test the possibility in comparison with the last suggestion of the preceding paragraph.

Twenty previously unused rats were selected and submitted to the usual training procedure. One week after they began to run the maze successfully and 2 weeks after the beginning of their training each of them was injected intraperitoneally with morphine hydrochloride, 13 mgm. per kgm., and their performance timed in the usual way. Ten of these rats continued to run the maze daily and were only injected again at the end of 4 and 8 weeks (Table VII). The other group of 10 animals (Table VIII) was taken off the maze after its timed record was obtained but was injected weekly with 13 mgm. per kgm. of morphine hydrochloride. At the end of 3 weeks, these rats were put back on the maze in order that a record of their performance could be obtained in connection with the fourth injection. In other words one group was run continuously but injected in the first, fifth and ninth weeks, whereas the other group was injected weekly but run and timed intermittently in connection with the first, fifth and ninth injections. In spite of this difference, both groups developed a decrease in the lengthening effect of morphine on the 'initial delay,' shown most markedly in the group averages and in the number of 'no starts' observed per day. This change was not greater in the group which was injected weekly than in the group which received only 3 injections in 2 mo.

It is important to note that in the 3-week intervals during which one

TABLE XI

SUMMARY: RATS SHOWING MEMORY OF MAZE OVER TWO PERIODS OF THREE WEEKS EACH, WHEN RATS WERE OFF THE MAZE, IN SPITE OF WEEKLY INJECTION OF MORPHINE

Date (1932)	Av. per run			Total per day		Remarks
	Initial delay	Active time	Waste time	Stops	Errors	
April 4	6.1	9.5	2.5	3	11	Day before first injection
" 27	9.6	12.1	25.2	21	36	Day put back on maze
" 28	7.2	10.1	2.3	7	5	
" 29	9.0	10.0	3.5	16	5	
May 2	5.1	9.5	1.0	2	1	Day before fifth injection
" 25	7.8	9.5	2.7	3	4	Day put back on maze
" 26	6.6	9.0	0.7	2	0	
" 27	5.3	8.7	1.0	2	1	
" 30	5.8	8.4	0.9	3	0	Day before ninth injection

TABLE XII

EFFECT OF SUBCUTANEOUS ADMINISTRATION OF MORPHINE HYDROCHLORIDE IN RATS PREVIOUSLY RUNNING THE MAZE REGULARLY FOR SIX OR MORE WEEKS AND RECEIVING SEVERAL INJECTIONS OF MORPHINE OR ALLIED SUBSTANCE AT WEEKLY INTERVALS DURING THAT TIME

Rat no.	Day	Dose (mgm. per kilo.)	Av. per run			Total per day				No. rats affected
			Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	22.9	7.7	6.1	44	12	0	0	—
17	Injection	3	40.6+	7.4*	2.8*	29	1	2	0	6
	Following	—	33.7+	7.5	9.9	72	19	1	0	—
	Previous	—	20.2	9.0	5.6	107	14	0	0	—
31	Injection	4	28.4+	8.8	1.6+	54	9	5	2	8
	Following	—	20.0	8.4	6.7+	109	18	0	3	—
	Previous	—	19.8	7.9	3.4	90	10	0	0	—
33	Injection	5	44.3+	8.0	3.4+	78	3	9	3	17
	Following	—	23.5	8.2	4.1	94	4	0	0	—
	Previous	—	24.8	8.0	3.3	93	5	0	0	—
34	Injection	6	49.1+	8.3	3.6+	74	0	8	6	18
	Following	—	24.2	8.2	6.5	125	20	0	0	—
	Previous	—	23.9	9.1	10.3	93	14	0	0	—
30	Injection	7	47.2+	9.3	8.1+	80	13	14	4	17
	Following	—	30.5+	8.2	11.0	138	10	1	0	—
	Previous	—	9.4	9.0	2.1	59	3	0	0	—
36	Injection	10	48.2+	10.8	4.2+	77	2	25	5	22
	Following	—	16.6+	9.5	3.7+	107	6	2	3	—

* Figures for active time and waste time are averages of completed runs only.

group of rats was off the maze the animals did not forget their previous experience and on the first day that they were put back on the maze ran it

almost as well as they had 3 weeks previously. To bring out this point Table XI summarizes the behavior of the group on the day before the injection of the first, fifth and ninth weeks and on the first 3 days after each of the 3-week periods when the rats were off the maze. The change in susceptibility evident in the group which was run for 4 weeks between injections seems to be associated with the repeated maze running, and it is entirely possible that weekly periods of maze-running developed the same change in the other group which was run only every fourth week, since, as has already been pointed out, the greatest change in susceptibility is associated with the first week of maze-running.

That little or no tolerance develops after many repeated weekly injections of morphine and allied substances is shown most clearly in Table XII, which records the effect of the same doses as in Table III on rats which had been injected many times. The similarity of results in these two tables, where the only difference is whether the dose recorded is the first which the rats received or a later one, seems to eliminate tolerance as a significant factor in the decrease in susceptibility of rats to morphine observed under certain conditions.

All the evidence then suggests that the phenomenon of decreased susceptibility is associated not with tolerance nor with altered absorption, but with repeated maze-running and is almost entirely restricted to the first month of such running. If animals are given a preliminary period of 4-6 weeks of maze experience, are subsequently used for not more than 3 months, and are considered in large enough groups, comparability of results in different groups is assured.

Summary

(1) A method is described which measures quantitatively the behavior of rats on an elevated maze, when hunger is the stimulus to learning and successful performance.

(2) The feature of this behavior which is most uniformly affected under the influence of morphine is initial delay, the time between the placing of the rat on the maze and its start along the true path. This time is significantly lengthened by morphine and the factors which might contribute to the lengthening are discussed.

(3) The effects of graded doses of morphine on initial delay are recorded, establishing the minimal effective dose as 6 mgm. per kgm. of the hydrochloride.

STUDIES OF MORPHINE, CODEINE, AND THEIR DERIVATIVES

VI. The Measurement of the Central Effect of Codeine, Dihydrocodeine, and Their Isomers by the Use of Maze-Trained Rats

By Nathan B. Eddy and Bertha Ahrens, University of Michigan

In the preceding paper of this series[1] the behavior on an elevated maze of rats under normal conditions and under the influence of morphine is described and discussed. The most significant change in the morphinized animals is the lengthening of *initial delay,* and it is believed that this lengthening may be used as a measure of effect on the central nervous system. The minimal effective dose of morphine hydrochloride evaluated by this method is 6 mgm. per kilogram, equivalent to 4.5 mgm. of alkaloidal base. Using the same maze-method, the relative effectiveness of codeine, dihydrocodeine, and their isomers, has been determined and the results are summarized in Tables I-VIII.

Reference should be made to the previous description for the details of the method and for the definition of terms, such as 'no-start,' 'incomplete run,' etc.; but it is necessary to point out that while the figures under the heading 'initial delay' there average the observations on all animals injected, the figures under the headings 'active time' and 'waste time' average only the observations on completed runs. Probably this excludes the rats which are most affected, but it is impossible to obtain a true figure for active or waste time when an animal fails to complete a run. If one multiplies the number of animals in any group by three and subtracts the sum of the 'no-starts' and 'incomplete runs,' one finds the number of runs completed. The figures for stops and errors also include only the data on completed runs, and inasmuch as these figures are totals per day, in a few instances a better comparison is obtained by dividing the number in each table by the number of completed runs. For example, in Table I of the present article, in the group of 22 animals to which

* Accepted for publication July 14, 1934. From the Laboratory of Pharmacology, University of Michigan Medical School. The work here reported is part of a unification of effort by a number of agencies having responsibility for the solution of the problem of drug addiction. The organizations taking part are the Rockefeller Foundation, the National Research Council, the United States Public Health Service, the United States Bureau of Narcotics, the University of Virginia, and the University of Michigan.

[1] A. K. Simon and N. B. Eddy, V. The use of maze-trained rats to study the effect on the central nervous system of morphine and related substances, this Journal, 47, 1935, 597-613.

TABLE I

CODEINE HYDROCHLORIDE

No. rats	Day	Dose (mgm. per kilo.)	Mode of administ'n	Av. per run			Total per day				No. Rats affected
				Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	—	20.9	11.3	11.1	143	5	0	0	—
20	Injection	10	S†	25.8+	11.0	22.5+	134	10	1	3	5
	Following	—	—	32.6	11.0	24.0+	135	8	0	3	5
	Previous	—	—	23.0	11.6	19.1	131	14	0	0	—
21	Injection	15	S	34.0+	11.1	27.2+	144	18	6	3	5
	Following	—	—	48.6+	11.1	46.2+	157	16	7	4	11
	Previous	—	—	18.5	10.8	6.0	84	2	0	0	—
22	Injection	18	S	41.1+	11.3	17.8+	79	8	15	2	8
	Following	—	—	21.9+	11.4	12.2+	153	8	1	2	4
	Previous	—	—	9.3	13.4	4.1	33	2	0	0	—
13	Injection	19	S	16.6+	14.2	7.5	53	2	1	0	2
	Following	—	—	9.0	11.9	2.9	28	0	0	0	1
	Previous	—	—	14.4	10.9	7.0	99	9	0	0	—
21	Injection	20	S	42.3+	12.1	18.6+	134	7	7	9	14
	Following	—	—	33.0+	12.3	28.3+	199	25	2	4	8
	Previous	—	—	16.6	10.1	10.0	119	2	0	0	—
20	Injection	21	S	51.8+	12.7	19.3+	117	1	10	6	14
	Following	—	—	18.6	10.1	5.8	79	4	0	0	2
	Previous	—	—	19.3	11.7	15.1	134	24	0	0	—
21	Injection	25	S	63.0+	13.7	22.4+	88	6	14	25	14
	Following	—	—	23.0	11.3	15.0+	134	25	0	5	5
	Previous	—	—	5.1	8.8	1.9	11	1	0	0	—
11	Injection	20	I§	15.5+	9.2	4.1	36	1	1	0	2
	Following	—	—	6.6	9.2	4.1	23	1	0	0	—
	Previous	—	—	9.1	11.8	2.4	26	0	0	0	—
11	Injection	23	I	14.5	12.5	6.9	42	3	0	0	2
	Following	—	—	10.6	14.8	5.1	22	2	0	0	—
	Previous	—	—	4.8	8.8	3.0	22	2	0	0	—
11	Injection	24	I	25.1+	9.8	19.9+	28	2	2	4	5
	Following	—	—	9.1	9.4	3.0	22	0	0	0	—
	Previous	—	—	12.7	10.4	13.6	69	6	0	0	—
11	Injection	25	I	107.1+	13.1	56.9+	—*	—*	25	6	11
	Following	—	—	22.9	11.1	12.2	66	6	0	0	—

* Only three completed runs on which 5 stops and one error were made

† S = subcutaneous

§ I = intraperitoneal

18 mgm. per kilogram of codeine hydrochloride were administered, 84 stops are recorded for the previous day and 79 stops for the day of injection. Changing these figures to averages for completed runs one gets 1.27 stops per rat per run on the previous day and 1.61 stops per rat per run on the day of injection, an increase of 26% in the stops under the influence of this dose of codeine.

TABLE II

ISOCODEINE ACID TARTRATE

No. rats	Day	Dose (mgm. per kilo.)	Mode of administ'n	Av. per run			Total per day				No. rats affected
				Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	—	16.6	9.3	9.3	40	13	0	0	—
10	Injection	60	S	15.6	8.4	3.3	15	1	0	0	0
	Following	—	—	19.0	10.0	5.9	26	4	0	1	1
	Previous	—	—	15.6	8.7	5.3	25	1	0	0	—
10	Injection	80	S	14.6	9.4	6.3	36	2	0	0	0
	Following	—	—	21.6	8.7	7.4	29	3	0	0	2
	Previous	—	—	14.5	11.6	8.2	34	6	0	0	—
13	Injection	100	S	23.6+	12.2	9.7	51	4	2	0	2
	Following	—	—	22.4+	13.5	11.0	41	7	1	1	1
	Previous	—	—	18.0	8.0	5.7	19	3	0	0	—
10	Injection	120	S	36.6+	8.0	1.8	7	0	4	2	4
	Following	—	—	35.6+	9.6	11.9	41	2	1	3	4
	Previous	—	—	6.8	14.4	0.9	3	0	0	0	—
5	Injection	60	I	11.6	14.6	2.1	0	0	0	0	1
	Following	—	—	9.4	15.1	1.4	0	0	0	0	—
	Previous	—	—	8.5	12.7	4.6	48	13	0	0	—
16	Injection	70	I	35.0+	16.1	6.1	46	6	5	0	8
	Following	—	—	14.4+	13.3	8.5	59	19	1	3	—
	Previous	—	—	7.1	12.4	2.3	19	2	0	0	—
16	Injection	80	I	20.3+	14.7	6.3	51	9	3	0	3
	Following	—	—	18.9	13.7	3.1	19	6	0	0	—
	Previous	—	—	7.6	12.7	1.9	13	2	0	0	—
16	Injection	90	I	34.8+	14.7	5.3	54	3	6	0	10
	Following	—	—	10.9	14.0	4.6	37	6	0	0	—
	Previous	—	—	9.8	16.2	2.3	2	6	0	0	—
5	Injection	100	I	67.4+	16.6	5.9	15	2	5	1	4
	Following	—	—	7.0	16.0	1.9	10	4	0	0	—

Codeine, dihydrocodeine, and their isomers produced effects on the maze-trained rats qualitatively like those which have been described for morphine.[2] Quantitatively, however, they are all weaker in their action than

[2] *Op. cit.*

morphine and stand in descending order of effectiveness codeine, isocodeine, allopseudocodeine, and pseudocodeine for the one group, and dihydrocodeine, dihydroisocodeine, dihydroallopseudocodeine and dihydropseudo-

TABLE III

PSEUDOCODEINE HYDROCHLORIDE

No. rats	Day	Dose (mgm. per kilo.)	Mode of administ'n	Av. per run			Total per day				No. rats affected
				Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	—	12.1	9.3	1.5	3	0	0	0	—
11	Injection	10	S	8.5	10.7	1.4	6	1	0	0	0
	Following	—	—	10.2	9.7	1.4	4	0	0	0	—
	Previous	—	—	15.7	10.6	6.5	49	2	0	0	—
10	Injection	20	S	15.8	10.4	8.2	48	2	0	0	0
	Following	—	—	20.0	10.2	8.9	67	0	0	0	2
	Previous	—	—	9.7	9.7	4.2	55	1	0	0	—
10	Injection	50	S	18.9+	10.7	6.0	46	0	2	0	1
	Following	—	—	35.0+	16.3	24.6	103	8	2	0	5
	Previous	—	—	19.0	8.3	5.1	33	2	0	0	—
11	Injection	100	S	27.0+	10.0	13.7	31	0	1	1	3
	Following	—	—	37.6+	11.8	20.9	63	10	3	1	4
	Previous	—	—	5.6	11.4	2.3	23	0	0	0	—
13	Injection	150	S	8.4	13.1	3.5	29	0	0	0	0
	Following	—	—	8.9	14.3	3.4	42	1	0	0	0
	Previous	—	—	8.2	12.6	2.3	23	1	0	0	—
12	Injection	200	S	10.6	13.8	4.1	23	0	0	0	2
	Following	—	—	9.5	16.6	6.6	44	2	0	0	1
	Previous	—	—	5.8	6.6	2.4	2	3	0	0	—
5	Injection	50	I	6.3	6.9	1.2	3	0	0	0	0
	Following	—	—	4.5	6.6	1.2	2	0	0	0	—
	Previous	—	—	8.9	7.3	2.6	0	2	0	0	—
5	Injection	80	I	10.5	7.6	0.3	3	0	0	0	1
	Following	—	—	Runs not recorded							
	Previous	—	—	20.8	11.4	6.0	34	3	0	0	—
13	Injection	90	I	47.1+	15.3	21.0	54	3	8	0	5
	Following	—	—	20.6	11.8	5.9	50	7	0	0	0
	Previous	—	—	7.7	10.9	2.2	10	0	0	0	—
9	Injection	100	I	60.6+	29.3	19.7	69	1	7	1	8
	Following	—	—	8.3	11.7	2.7	13	1	0	0	0

codeine for the group of hydrogenated derivatives (see Table IX below). The criterion which has been adopted for the establishment of relative effectiveness is the average of the initial delays for the 3 runs on the day

of injection in comparison with the average of the initial delays for the 3 runs on the previous day. An animal is considered to be affected ('depressed') when the injection-day average is at least double that of the

TABLE IV

ALLOPSEUDOCODEINE HYDROCHLORIDE

No. rats	Day	Dose (mgm. per kilo.)	Mode of administ'n	Av. per run			Total per day				No. rats affected
				Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	—	10.2	10.8	2.4	18	2	0	0	—
11	Injection	10	S	8.5+	13.1	5.4	10	1	1	0	0
	Following	—	—	8.0	11.5	1.7	7	1	0	0	0
	Previous	—	—	10.1	8.9	3.3	9	0	0	0	—
6	Injection	50	S	10.6	10.2	6.7	18	0	0	0	0
	Following	—	—	15.3	10.9	6.3	28	1	0	0	1
	Previous	—	—	17.5	8.5	4.8	8	0	0	0	—
7	Injection	100	S	9.5	10.3	1.2	5	0	0	0	0
	Following	—	—	9.4	9.5	1.7	10	2	0	0	0
	Previous	—	—	6.9	9.1	1.4	2	1	0	0	—
12	Injection	200	S	9.2	10.3	3.6	20	0	0	0	1
	Following	—	—	8.7	9.6	1.5	6	1	0	0	0
	Previous	—	—	6.1	10.7	1.3	2	0	0	0	—
7	Injection	300	S	7.0	13.2	1.8	6	0	0	0	0
	Following	—	—	21.1+	13.7	19.4	32	5	1	0	1
	Previous	—	—	9.4	20.4	3.8	8	2	0	0	—
5	Injection	70	I	17.7	20.7	5.5	18	0	0	0	1
	Following	—	—	9.1	18.2	1.1	1	0	0	0	0
	Previous	—	—	9.9	10.4	4.5	45	2	0	0	—
11	Injection	80	I	23.6+	15.0	17.5	95	1	1	0	4
	Following	—	—	11.5	10.7	3.6	39	1	0	0	0
	Previous	—	—	11.2	9.8	5.8	53	3	0	0	—
11	Injection	90	I	83.5+	14.3	37.1+	71	2	14	5	10
	Following	—	—	17.8	10.7	11.3	73	3	0	0	1
	Previous	—	—	8.8	12.5	2.9	18	0	0	0	—
10	Injection	100	I	27.0	19.9	21.4+	104	0	0	3	6
	Following	—	—	8.2	12.3	3.5	24	4	0	0	0
	Previous	—	—	5.8	17.3	1.0	5	0	0	0	—
5	Injection	120	I	80.4+	46.5	14.8	3	2	6	0	5
	Following	—	—	15.4	24.2	2.2	7	1	0	0	2

previous day and at the same time exceeds the latter by at least 15 sec. The minimal effective dose of any drug then is the smallest dose which affects to the extent just described the majority of the group of rats receiving that dose.

In the administration of morphine to maze-trained rats, it was found that more uniform results were obtained at the arbitrarily selected 1-hr. period of observation if the drug was given subcutaneously than if it was

TABLE V

DIHYDROCODEINE ACID TARTRATE

No. rats	Day	Dose (mgm. per kilo.)	Mode of administ'n	Av. per run			Total per day				No. rats affected
				Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	—	6.9	11.6	1.8	18	0	0	0	—
13	Injection	10	S	6.9	12.1	3.3	28	0	0	0	1
	Following	—	—	6.0	10.9	6.1+	18	3	0	1	0
	Previous	—	—	10.6	9.4	2.7	17	2	0	0	—
10	Injection	15	S	23.9+	9.8	17.6+	38	3	1	2	3
	Following	—	—	21.0+	8.4	13.8	29	4	1	0	3
	Previous	—	—	26.0	12.1	4.5	115	15	0	0	—
10	Injection	20	S	46.3+	11.7	3.1+	71	3	4	3	4
	Following	—	—	23.6	14.1	2.0+	90	7	0	2	1
	Previous	—	—	7.3	11.6	4.9	42	2	0	0	—
13	Injection	21	S	31.6+	12.9	23.0+	70	3	5	2	3
	Following	—	—	8.0	12.1	3.4	44	2	0	0	0
	Previous	—	—	19.3	10.4	17.5	78	4	0	0	—
9	Injection	22	S	46.3+	9.3	20.9+	69	6	2	2	6
	Following	—	—	33.6+	10.9	27.1+	80	13	1	1	3
	Previous	—	—	15.3	9.8	13.7	31	11	0	0	—
10	Injection	23	S	42.0+	10.7	13.0+	49	1	1	2	7
	Following	—	—	17.0	9.2	6.2	36	4	0	0	1
	Previous	—	—	9.0	11.2	3.6	39	3	0	0	—
20	Injection	20	I	12.3	11.1	3.7	45	1	0	0	2
	Following	—	—	9.7	11.8	4.7	53	6	0	0	0
	Previous	—	—	19.5	12.0	8.0	75	6	0	0	—
21	Injection	21	I	47.4	15.0	7.5	84	5	14	2	8
	Following	—	—	15.3	11.6	7.0	88	1	0	2	3
	Previous	—	—	13.7	10.6	7.0	47	2	0	0	—
11	Injection	22	I	63.7+	10.1	8.9+	48	2	9	1	10
	Following	—	—	22.4	10.7	11.0+	58	4	0	1	2

injected intraperitoneally. Nevertheless, in the present series of experiments with all of the drugs except codeine, dihydrocodeine and dihrydroisocodeine, absorption from the subcutaneous tissues appeared to be very slow so that difficulty was experienced in finding doses which were effective in terms of the criterion described above. Subcutaneous doses of these substances larger than those recorded in the tables were not given, partly because of

tissue-destruction at the site of injection and partly because of the development of after-effects. The tissue-destruction caused breaking down of the skin over the injected area during the first 24 hr. with the formation of open sores which were very slow in healing. The delayed effect was seen in only a part of the rats but gave a picture on the day after injection qualitatively typical of the acute effect of morphine. Probably this dis-

TABLE VI

DIHYDROISOCODEINE ACID TARTRATE

No. rats	Day	Dose (mgm. per kilo.)	Mode of administ'n	Av. per run			Total per day				No. rats affected
				Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	—	10.2	12.1	3.4	40	0	0	0	—
10	Injection	40	S	12.2	12.8	4.8+	39	0	0	3	2
	Following	—	—	11.9	11.3	4.2	41	0	0	0	0
	Previous	—	—	6.9	11.8	1.9	16	0	0	0	—
11	Injection	50	S	11.8	12.1	4.5+	36	0	0	3	2
	Following	—	—	8.8	12.5	3.6	33	2	0	0	0
	Previous	—	—	16.9	9.8	3.4	27	4	0	0	—
12	Injection	60	S	39.9+	11.4	8.7	50	1	3	0	6
	Following	—	—	19.4	9.3	4.4	35	0	0	0	0
	Previous	—	—	16.1	11.1	6.5	54	1	0	0	—
11	Injection	70	S	54.5+	15.0	19.2+	97	2	3	1	8
	Following	—	—	18.5	11.3	5.4	44	0	0	0	0
	Previous	—	—	5.9	11.6	6.1	43	9	0	0	—
16	Injection	40	I	33.3+	11.2	4.0+	51	0	9	3	5
	Following	—	—	11.0	12.7	4.2	51	1	0	0	2
	Previous	—	—	11.0	11.7	5.2	56	2	0	0	—
16	Injection	50	I	83.2+	17.4	9.1	27	1	30	0	12
	Following	—	—	16.0	11.0	13.4+	101	22	0	3	2
	Previous	—	—	10.4	10.8	11.2	90	9	0	0	—
10	Injection	60	I	100.6+	14.2	62.0+	25	2	23	4	10
	Following	—	—	15.5+	9.7	10.7+	50	5	2	3	2

turbance of behavior on the days after injection was contributed to by the presence of the open sores but it was seen frequently after codeine, which did not cause tissue-destruction in the doses used. No such difficulties were encountered when the drugs under consideration were injected intraperitoneally. Also after this mode of administration, disturbance of behavior on the following day was rare.

With the exception of dihydropseudocodeine, each of the hydrogenated derivatives were more effective by the present method of valuation than

the corresponding codeine isomer. This is in accord with previously reported observations[3] in regard to the relative activity of the hydrogenated member of a pair of alkaloids. On the other hand, it has been found that allopseudocodeine in toxic doses in rabbits and mice exhibited a high degree of convulsant action and showed little evidence of previous de-

TABLE VII

DIHYDROPSEUDOCODEINE HYDROCHLORIDE

No. rats	Day	Dose (mgm. per kilo.)	Mode of administ'n	Av. per run			Total per day				No. rats affected
				Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	—	16.4	11.3	1.9	6	3	0	0	—
9	Injection	10	S	24.8+	10.5	1.6	6	0	3	0	1
	Following	—	—	25.1+	10.6	1.9	6	0	3	0	1
	Previous	—	—	13.4	10.5	2.2	33	1	0	0	—
7	Injection	50	S	12.5	11.2	2.2	10	0	0	0	0
	Following	—	—	12.1	11.5	1.4	4	0	0	0	0
	Previous	—	—	10.2	12.9	3.5	17	2	0	0	—
10	Injection	100	S	8.4	15.6	4.0	34	0	0	0	0
	Following	—	—	11.9	17.2	6.6	44	1	0	0	0
	Previous	—	—	5.9	11.4	3.7	13	4	0	0	—
10	Injection	150	S	7.3	14.7	15.8	63	4	0	0	2
	Following	—	—	32.1+	18.9	35.1+	102	12	1	1	8
	Previous	—	—	8.8	18.5	1.9	4	1	0	0	—
5	Injection	50	I	7.3	18.0	1.1	1	0	0	0	0
	Following	—	—	13.5	15.3	23.8	12	39	0	3	1
	Previous	—	—	8.0	10.9	3.6	61	1	0	0	—
24	Injection	100	I	25.0+	16.0	8.9+	77	2	4	3	10
	Following	—	—	17.7	12.9	15.5	111	11	0	0	3
	Previous	—	—	13.2	10.0	9.3	61	13	0	0	—
11	Injection	120	I	23.1	12.5	5.6+	48	0	0	2	5
	Following	—	—	10.0	10.0	4.0	30	4	0	0	0
	Previous	—	—	9.9	9.5	4.3	18	5	0	0	—
11	Injection	130	I	58.9+	15.2	31.0+	48	0	7	5	8
	Following	—	—	11.7	10.9	4.1	39	0	0	0	0

pression,[4] while pseudocodeine produced prolonged depression and never caused convulsions. Also dihydroallopseudocodeine, although it was more depressant than allopseudocodeine, was still convulsant with fatal doses, while dihydropseudocodeine, like pseudocodeine, was non-convulsant. In

[3] N. B. Eddy, Hydrogenated codeine isomers, *J. Pharm. & Exper. Therap.*, 51, 1934, 35-44; C. I. Wright, The respira-effects of morphine, codeine, and related substances, *idem*, 343-352.

[4] Eddy, Isomers of codeine, *ibid.*, 45, 1932, 361-381.

TABLE VIII

DIHYDROALLOPSEUDOCODEINE ACID TARTRATE

No. rats	Day	Dose (mgm. per kilo.)	Mode of administ'n	Av. per run			Total per day				No. rats affected
				Initial delay	Active time	Waste time	Stops	Errors	No starts	Incomplete runs	
	Previous	—	—	13.3	9.7	2.6	11	0	0	0	—
10	Injection	10	S	10.0	9.0	1.3	2	0	0	0	0
	Following	—	—	11.7	9.1	1.0	0	0	0	0	0
	Previous	—	—	6.0	11.1	2.4	21	7	0	0	—
11	Injection	50	S	6.3	10.8	2.1	14	2	0	0	0
	Following	—	—	7.9	11.7	3.8	26	3	0	0	0
	Previous	—	—	28.7	14.1	11.1	60	2	0	0	—
10	Injection	100	S	47.7+	17.4	26.7+	84	7	3	2	4
	Following	—	—	41.1+	13.1	17.1+	52	13	7	2	3
	Previous	—	—	11.6	14.4	6.2	41	0	0	0	—
10	Injection	150	S	15.0	14.3	6.8	35	0	0	0	1
	Following	—	—	12.9	14.2	15.6	50	3	0	0	0
	Previous	—	—	13.7	11.1	5.8	19	4	0	0	—
8	Injection	50	I	10.7	10.4	2.0	10	0	0	0	0
	Following	—	—	10.7	10.7	2.0	14	0	0	0	0
	Previous	—	—	9.7	12.7	6.2	26	6	0	0	—
10	Injection	80	I	40.2+	13.9	26.5+	49	7	2	3	4
	Following	—	—	27.9	13.7	9.3+	45	1	0	1	2
	Previous	—	—	15.8	10.9	8.0	69	7	0	0	—
17	Injection	100	I	44.5+	12.8	13.1+	93	2	11	2	7
	Following	—	—	18.1	11.1	8.4	85	6	0	0	2
	Previous	—	—	26.7	10.3	27.1	80	9	0	0	—
11	Injection	120	I	80.8+	11.7	31.6+	53	2	14	1	11
	Following	—	—	16.7	10.4	8.3	46	0	0	0	1

TABLE IX

MINIMAL EFFECTIVE DOSES (MGM. PER KILO.) OF ALKALOIDAL BASE

	Subcutaneous administration	Intraperitoneal administration
Codeine	16.09	20.12
Dihydrocodeine	14.19	14.19
Isocodeine	More than 120.00	58.89
Dihydroisocodeine	41.79	29.85
Pseudocodeine	More than 200.00	89.13
Dihydropseudocodeine	More than 150.00	110.24
Allopseudocodeine	More than 300.00	80.21
Dihydroallopseudocodeine	More than 150.00	74.16

view of these observations it is quite unexpected to find that, in the rat, allopseudocodeine had a greater depressant effect than pseudocodeine as measured by the lengthening of initial delay and that dihydroallopseudocodeine had a greater effect than dihydropseudocodeine.

Summary

(1) When administered in sufficient dose and by a suitable channel, each of the codeine and dihydrocodeine isomers affects the maze-trained rat qualitatively like morphine. They are all quantitatively weaker in action than morphine.

(2) The descending order of effectiveness of the codeine isomers is codeine, isocodeine, allopseudocodeine and pseudocodeine. The descending order of effectiveness of the dihydrocoeine isomers is dihydrocodeine, dihydroisocodeine, dihydroallopseudocodeine and dihydropseudocodeine.

(3) With the exception of dihydropseudocodeine, the hydrogenated derivative is more effective than the corresponding unhydrogenated compound, confirming in this way previous observations on increase in depressant action by hydrogenation in the morphine series.

FACTORS DETERMINING THE CHARACTERIZATION OF MUSICAL PHRASES

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Experiments dealing with the psychological factors relating to music have in general been confined either to the physiological changes that accompany various types of music or to the various ways in which auditors listen to, or are affected by, music.[1] Experimenters have usually assumed that there are 'sad,' 'glad,' and other types of music, and have never stopped to determine in any objective way the distinguishing characteristics of such mood-determining sounds. This paper presents a pioneering effort directed toward establishing some of the compositional factors within music that fluctuate more or less closely with its character.[2]

Let us consider the range of expression that music commands. At the zero extreme stands that music which for a fairly large part of its audience conveys no mood and has little or no character, but only serves, let us say, as a vehicle for the troubadour or as a decoration for the words of a ballad. Many compositions may be found that have no definitive character. There is considerable evidence to show, however, that many unfamiliar concert-selections can arouse a fairly uniform mood and are characterized in much the same manner by a large body of hearers. Beyond this the agreement trails off. Those who listen without program notes to relatively unfamiliar concert-music do not experience common ideas or think of the same situations. Musical programs provide many concert-listeners an ex-

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[1] See, *e.g.* C. M. Discerns, *The Influence of Music on Behavior*, 1926, 1-224; M. Schoen (ed.), *The Effects of Music*, 1927, 1-273; W. V. Bingham, Studies in melody, *Psychol. Monog.*, 12, 1910, (no. 50), 1-88; H. P. Weld, An experimental study of musical enjoyment, this JOURNAL, 23, 1912, 245-308; and C. S. Myers, Individual differences in listening to music, *Brit. J. Psychol.*, 13, 1922, 52-71. A more general account of music, in relation to aesthetics and involving the question of tests for musical capacity as well as problems relating to musical composition, may be found in C. C. Pratt, *The Meaning of Music*, 1931, 1-245.

[2] A treatment of melodic line somewhat after the approach suggested in this paper may be found in Otto Ortman, On the melodic relativity of tones, *Psychol. Monog.*, 35, 1926, (no. 162), 1-47.

cellent opportunity to day-dream uninterruptedly for the duration of the performance. The music influences somewhat consistently from person to person the direction and pace of the imagery, although the objects of the imagery may be determined, as in other types of dreaming, largely by visceral conditions and by attitudes. The common effect of music, then, is not ideational; but it can express a certain mood or temper, a certain character.

What determines the agreement? Consider that most of us who listen to music have sung songs, hymns, rounds, and perhaps even jazz, and that we are able to recognize these conventionalized types of music. Several centuries of musical tradition dictate to the composer certain general features that are followed in attempting to express a given mood. We are trained in a tradition. Some of us are of course much better trained than others in the meaning of the symbols, but all of us recognize the more basic ones. Even music has its clichés. The question now arises: Are these devices for expressing moods almost entirely arbitrary conventions, as is our spoken language; or are the devices rooted in biological fact, as is our facial expression of emotion? Such a problem could hardly be settled by an examination of the music of one culture alone.

A study has been published comparing certain characteristics of the music of six Indian tribes, designed to provide a basis of comparison between the expression of mood through music in the various Indian cultures and our own.[3] The probable quality of the Indian songs was established by a consideration of the occasion for their singing, such as 'triumphal return of a war party,' 'the death of a hero,' 'the death of a lover.' After an elaborate statistical analysis of the songs, comparisons between types of songs and between tribes were made. The songs of different mood-groups (classified by situation) showed a striking contrast, while comparisons from tribe to tribe of groups of songs representing the same situation showed considerable agreement. It was concluded that music has some conventions grounded on a firm basis of common physiological structure and behavioral similarity of human beings.

The specific results of such a study of Indian music could not provide a safe basis for predicting the specific conventions of Western European music. The music of the Indian consists of songs that form an integral part of his life in war, in sickness, in triumph, in love, and in games. Although a great deal of our life has a musical accompaniment—with the radio loud-speaking most of the day, the music at meetings of fraternal organizations,

[3] R. H. Gundlach, A quantitative analysis of Indian music, this JOURNAL, 44, 1932, 133-145.

religious services, rallies; with music as a background for dancing, eating, and many types of entertainment, and for some of us, even as a source of satisfaction in itself—music rarely has for members of our society the mystic potency and supernatural charm that is attributed to it by the more primitive Americans. Finally, while the Indian music is simply a vocal melodic line (with sometimes a percussive accompaniment), Western music may be vocal or instrumental, complexly harmonic as well as melodic.

The methods that proved feasible for analyzing the Indian music, however, are admirably suited for a comparable study of Western European music. The problem which this paper reports has three major parts; first, the determination of consistency and uniformity with which observers can report the quality characteristic of some piece; secondly, an analysis of the musical mechanics of the pieces that may be partly responsible for the mood-characterization; and, thirdly, an analysis of the interrelations of the various qualitative categories.

METHOD

Forty fairly diverse pieces electrically recorded were selected for presentation. To simplify the thematic material only the first few bars of each selection—a single musical phrase—was presented. Each phrase was played twice in immediate succession before groups of 20–40 auditors. The entire set of selections could be played and reported upon in approximately an hour. With short selections, and little time to reflect and judge, the reliability of the reports is doubtless lower than it would have been with a more leisurely development of the theme.

There was some debate as to whether the selections should be presented as recorded, whether symphonic, piano or choral, albeit constant from presentation to presentation and subject to duplication at any later time. The alternative to recorded music would have been a piano and a performer. This method would have introduced many complex factors of variations in speed and loudness, in interpretation, in expertness of rendition, and the like, that were considered more objectionable than the difficulties with the method finally pursued.[4]

The *O*s used were students in psychology and students in music, arranged into three groups according to their training. A group of 33 had no training and little or no knowledge of music. They were interested enough, however, to serve freely as *O*s. A second group of 25 had no theoretical training in music, and no formal training in musical history, but had received instrumental instruction, and had at some time

[4] It is interesting to note in this connection the mood-effects that can be indicated by a musician singing the same note for approximately the same duration of time and intensity. Sherman reports that sorrow and anger were quite uniformly recognized, while the other two categories used, fear-pain, and surprise, were less accurately recognized. Mandel Sherman, Emotional character of the singing voice, *J. Exper. Psychol.*, 11, 1928, 495-497.

played. The third group of 54 included only advanced students in music, and were taken from classes in musical composition and choral forms. What the effects of training would be upon the reports one would hardly dare predict, but allowance

TABLE I

List of Compositions in Order of Presentation

No.	Composer	Composition	Characterization
1.	Handel	Harmonious Blacksmith	*Dignified*
2.	Tschaikowsky	Symphonie IV (1st Movement)	Tranquil
3.	Tschaikowsky	Symphonie IV (3rd Movement)	*Animated*
4.	Mozart	Symphonie in G	
5.	Mendelssohn	Midsummer Night's Dream (Scherzo)	Whimsical
6.	Grieg	Nocturne	
7.	Grieg	March of the Dwarfs	*Animated*, grotesque
8.	Gregorian Chant	The Ordinary of the Mass (Part 1)	
9.	Ghys	Amaryllis	*Glad*, whimsical, flippant
10.	Eicheim	Japanese Nocturne	Mournful, grotesque
11.	Dukas	Sourcerer's Apprentice	
12.	Delius	Brigg Fair (Part 1)	*Tranquil*, sentimental
13.	Debussy	Golliwog's Cake-walk	Animated, *grotesque*
14.	Beethoven	Sonata 'Pathetique'	Dignified
15.	Dvorak	New World Symphony (Scherzo)	*Animated*
16.	Brahms	Symphonie I (Part 1)	
17.	Brahms	Symphonie I (Part 4)	Dignified
18.	Rimsky-Korsakov	Scheherazade (1st Movement)	Dignified, somber, triumphant
19.	Rimsky-Korsakov	Scheherazade (4th Movement)	Uneasy
20.	Berlioz	Symphonie 'Fantastique' (1st Movement)	
21.	Berlioz	Symphonie 'Fantastique' (5th Movement)	Grotesque
22.	Debussy	The Sea	Uneasy
23.	Bach	Brandenburg Concerto No. 2	*Brilliant, triumphant*, animated, *glad*
24.	Bach	Passacaglia	Somber
25.	Moussorgsky	A Night on the Bare Mountain (Part 1)	*Animated, uneasy*
26.	Moussorgsky	A Night on the Bare Mountain (Part 1, second face)	Triumphant
27.	Chopin	First Prelude	
28.	Chopin	Fourth Prelude	*Melancholy*
29.	Chopin	Seventh Prelude	*Delicate, tranquil*
30.	Chopin	Eleventh Prelude	Animated, glad, whimsical
31.	Chopin	Fifteenth Prelude	*Tranquil*
32.	Chopin	Sixteenth Prelude	*Brilliant, animated*
33.	Chopin	Nineteenth Prelude	Glad
34.	Chopin	Twenty-second Prelude	*Animated*
35.	Gershwin	Concerto in F (Part 1)	*Grotesque*
36.	Gershwin	Concerto in F (Part 2)	
37.	Haydn	Sonata in C Minor (Part 1)	
38.	Haydn	Sonata in C Minor (Part 2)	
39.	Herbert	Dagger Dance	*Triumphant*
40.	Herbert	Indian Summer	*Tranquil, sentimental*

for differences must certainly be made. The knowledge of the whole piece would doubtless determine the interpretation of the phrases.

The *Os* were provided with a mimeographed sheet which asked for information concerning their training, and provided the following instructions.

Please attend carefully to a number of short musical phrases that are to be played. Determine what mood or attitude the composer has succeeded in expressing. Let your judgment characterize the aspects of the music, regardless of your own feelings or likes. Express your judgment by marking in the column corresponding to the number of the composition those terms which correctly describe the musical phrase. If none of the printed categories adequately fit the phrase, write one that does, in the blank spaces. A musical phrase may correctly be characterized by more than one set of terms.

There followed a list of 17 categories and 8 blank spaces and a row of 40 columns in which to check the proper characteristics of the various pieces.

The *O*s were not asked what mood the music induced in them because such a statement of their task permitted a fusion of the effects of the music with factors in their own biography that might have distorted the reports in the direction of individual idiosyncracy. The directions tried to induce an objective characterizing attitude that would aim to abstract from the report factors attributable to items of presentation, to previous acquaintance and associations with the selection, and to extraneous metabolic and postural conditions. The *O*s were asked, then, to characterize the aspects of the music in terms similar to the categories presented on the data sheet.

The list of categories was worked out after prolonged consideration. It was thought essential to present a fairly complete list of categories to select from rather than to permit the *O*s to use whatever terms they happened upon at the moment of hearing a phrase. The terms selected were often paired, so that the two would indicate a range of meanings that would be more universally agreed upon than could, perhaps, a single term. The categories finally selected fell into three groups of somewhat different dimensions. One set related primarily to the energy or the dynamical (speed and power) properties of the music, listing five points in a series; namely, brilliant, animated or excited, uneasy or restless, tranquil or restful, and dignified or sedate. It was believed that every piece could be characterizable by one of these qualifications, in the same way that all visual qualities are by 'brightness.' The second series seemed to express some additional mood-characteristic, or emotionally toned attitude, but could almost be paired with the set of dynamical terms. These six include triumphant or victorious, sublime or exalted, glad or joyful, somber, melancholy or despondent, and mournful or bleak. The third set of qualifications was somewhat hard to designate, but seemed to involve a certain playful self-consciousness; an amused, detached, self-conscious, perhaps sophisticated attitude of the composer. The list, ranging from light to heavy, included delicate or tender, whimsical or fanciful, flippant or pert, sentimental or romantic, awkward or constrained, and grotesque. Whether there are such dimensions that can be used to represent definite aspects of music, and what particular terms belong in such dimensions are, of course, questions not to be settled by speculation. But the interrelationship of such categories seems to me to be a very interesting and provocative question and one that has been too much neglected. Some of the relationships discovered between these terms by the reports of 112 persons on 40 different musical phrases constitute the last section of this paper.

The musical scores of the forty pieces were analyzed and tallied in the following respects. (1) The loudness values were scored on a scale of from one to five by the writer and by Professor McKay. There was high agreement on the judgments. In cases of disagreement, the selection in question was played over, in series with

other pieces of agreed loudness, and mutually located. (2) Tempo was estimated with numbers being substituted for the five musical categories of *presto, allegro, mezzo, andante,* and *largo*. The two judges agreed in all but nine of the pieces, where Professor McKay's judgment took precedence. (3) The range of the melodic line was measured using eight notes to the octave. (4) The range of orchestral accompaniment was measured in the same fashion. (5) The relative pitch of the selections was determined by selecting the mid-most tone in the melodic line. The notes were translated into equivalent numbers for statistical purposes. (6) The successive steps in the melody line were tallied according to their size, and recorded in terms of relative proportion. Values for repetition of the same note, for seconds, thirds, fourths, and fifths, or larger, were separately recorded. (7) Following the analysis made with the Indian music, the rhythms were divided into three categories according to their flow. Smooth rhythms include units that move along using just the temporal duration, as in the *Doxology,* or more rapidly moving doublets, triplets, or quadruplets. Uneven rhythms involve the small rhythmic units that are easy flowing, but not equivalent, as in the customary waltz tune. Rough rhythms involve the vigorous jerking such as those parts of *Marseilles* or *Soliloquy* that are not smooth. (8) Records were kept of the direction of the melodic line and of the kind of instrument carrying the melodic line. The melodic line itself has shown no significance for our problem.

Results

The results may be discussed under three headings: *(1)* reliability of the reports; *(2)* the relation of the descriptive terms to the analyzed musical factors; and *(3)* the interrelations of the descriptive categories.

(1) Reliability. The list of composers and their compositions in order of presentation are catalogued in Table I, together with the terms that chiefly characterize the introductory phrase. A measure of the reliability of the reports is a measure of agreement among the *O*s that a piece expresses a certain character. A glance at the table summarizing the raw data (not here presented) reveals that there are 24 occasions where 50 or more of all the 112 *O*s agree that a certain piece expresses a certain character. This does not mean that 24 selections are characterized by so large a vote, for a single piece might be characterized by two or three of the categories.

Bach's *Brandenburg Concerto No. 2,* for instance, (Selection 23) shows the following: 66 vote it 'glad'; 51, 'brilliant'; 50, 'triumphant'; 37, 'animated'; and 10, 'exalted.' Only four of the 17 terms receive no vote for this selection. The first selection, Handel's *Harmonious Blacksmith,* received 63 votes for 'dignified,' and from 15 to 20 votes for 'triumphant,' 'glad,' 'exalted,' and 'tranquil.' Mozart's *Symphony in G* (Selection 4) received 29 votes for 'glad,' 26 for 'uneasy,' and 16 for 'whimsical.' Ten other 'mood' categories received from 4 to 11 votes for this selection. The characteristics that are listed for the compositions in Table I in italics had at least 50 out of the 112 possible votes. The remaining characterizations were not selected simply on the basis of the number of votes, but the number weighted in relation to the training of the *O*s. In order to determine the effects of training,

the data from the 54 highly trained *O*s were correlated with the reports of the 33 untrained. Using the rank-difference method, correlations were determined for each of the 40 selections between the votes of the trained and the untrained groups. The raw correlations show 10 over 0.90, 18 others above 0.80, and the remaining twelve below 0.79. Mozart's *Symphony in G* shows the greatest disagreement, and here the judgments of the trained and untrained *O* correlates but 0.41. Four other pieces show correlations below 0.70. Selection 3 (Tschaikowsky's *Symphony IV,* 3rd movement) has a correlation of 0.69 between the two groups. In the same way Selection 16 (Brahms' *First Symphony,* 1st movement), Selection 19 (Rimsky-Korsakov's *Scheherazade,* 4th movement), and Selection 34 (Chopin's Twenty-second *Prelude*) had correlations of 0.65, 0.59, and 0.59.

Let us view the reports, not from the point of view of the 40 selections, but from that of the 17 categories (Table II). Considering the entire set of reports, some of the categories are seen to be much more popular than others. While 'flippant' got a total of but 167 votes on all the selections, 'animated' received 581. The rank-orders for the votes of the total group, and for each of the three sub-groups of *O*s is presented in Table II. It may be seen that several of the categories are much more frequently used by one of the groups, and several other categories by a different group. Note especially the categories 'brilliant,' 'sentimental,' and 'mournful.' This table also presents the correlations between the musically trained and untrained groups. It is noteworthy that the wide differences in the use of the various musical categories by the different groups of *O*s has a relationship as low as 0.30 to the size of the correlations. There are but two correlations in the sixties ('uneasy' and 'mournful'), and but one below that figure ('exalted,' 0.41). For convenience some of the data presented in Table I are reproduced here in another form. The order-of-presentation number of those selections that were definitely characterized by some category is listed after the appropriate mood-characterization. The five characterizations that were considered 'dynamical' are among the most used categories and typify a relatively large number of pieces, 'brilliant,' 'animated,' 'uneasy,' 'tranquil,' and 'dignified.' The categories 'glad' and 'triumphant' seem, however, very closely related, and 'brilliant' seems less related to the remaining 4 of the dynamical terms.

Five correlations between the reports of two sections of the untrained group and five for two sections of the highly trained group on reports on the same pieces were all comparable in size to the correlations between the trained and untrained groups. It is surprising to find no greater consistency among the musically trained than among the untrained. The chief effects of musical training that have so far been discovered consist in increasing the total number of reports made about 15%, and by changing the frequency of usage of certain of the categories.

Reliability coefficients for the various moods (see Table V) were computed, taking care to split the various training groups. Only one of the rank-difference correlations is below 0.75. This is the reliability of the term 'exalted,' whose coefficient is 0.54. Ten of the coefficients are above 0.85.

We may conclude that the reliability of judgments concerning the character of a piece depends somewhat upon the training of the *O*s, but probably in most cases more upon the nature of the piece and the categories that are used to describe it. With regard to the present selection of musical phrases and categories, the reliability of the judgments of our observers was of satisfactory magnitude.

(2) Relation of categories to musical mechanics. Two methods of attack are open in studying the dependence of the character of music upon the specific musical structures of the selections. One method consists in finding a number of selections that are characterized by a term, and then determining the significant musical factors for this group of selections. These values may be compared either with the average values of all the 40 selections, or they may be compared with that group of the selections which the observers agreed did not express the term in question. The other approach consists in starting, not from the characterization, but from the musical

TABLE II

RANK ORDER OF THE TERMS USED IN CHARACTERIZING THE MUSICAL SELECTIONS FOR THE TRAINED, SEMI-TRAINED, AND UNTRAINED Os, AND FOR THE GROUP AS A WHOLE, TOGETHER WITH THE CORRELATION OF THE RANK ORDERS OF THE TRAINED AND UNTRAINED Os

Terms	Rank order				Correlation between trained and untrained Os	No. of musical selection thus characterized
	total (112)	trained (54)	semi-trained (25)	un-trained (33)		
Animated	1	1	3	1	.91	3, 7, 13, 15, 23, 25, 30, 32, 34
Grotesque	10	11	7	10	.86	7, 10, 13, 21, 35
Brilliant	7	3	8	16	.86	23, 32
Delicate	12	8	12	13.5	.85	29
Glad	5	6	9	9	.84	9, 23, 30, 33
Melancholy	9	13	10	8	.84	28
Tranquil	3	4	1.5	5	.79	2, 12, 29, 31, 40
Whimsical	8	10	5	7	.78	5, 9, 30
Flippant	17	16	17	17	.77	9
Awkward	16	17	15	15	.77	
Dignified	4	5	1.5	6	.73	1, 14, 17, 18
Triumphant	14	14	13	11	.72	18, 23, 26, 39
Somber	6	9	4	4	.71	18, 24
Sentimental	13	7	14	12	.70	12, 40
Uneasy	2	2	6	2	.68	19, 22, 25
Mournful	11	12	11	3	.63	10
Exalted	15	15	16	13.5	.41	

analyses. For each factor, loudness, speed, range, and the like, the extreme pieces can be picked and compared for the types of category expressed. One could compare, say, the loudest 10 with the quietest 10. Will there be the same number of "dignified" selections in each of these quartiles? If not, is the excess in one significantly greater than the deficiency in the other? The first of these methods provides greater precision of report; the second method has been employed in this survey as a check.

The selection of pieces to be used in establishing the musical characteristics of some category is governed by contrasting factors. There is need of compositions consistently inspected to express the category, on the one hand, and the necessity of enough compositions, on the other hand, to eliminate in part the unessential and

TABLE III

SIGNIFICANT MEAN VALUES AND STANDARD DEVIATIONS FOR GROUPS OF PHRASES CHARACTERIZED BY A TERM

Terms	Tempo	Rhythms (percentages)			Intervals (percentages)					Orchestral range	Loudness	Mean pitch	Melodic range	Phrases	
		Smooth	Uneven	Rough	1st	2nd	3rd	4th	5th					Characterized	Not characterized
Animated	4.63	53	23							24.4	3.45	3.54		11	13
	.47	43	37							6.7	.78	.33			
Brilliant	4.66	58	30						10		3.33		11.84	12	14
	.47	45	42						13		.68		4.60		
Dignified	2.27	21	67					9		19.5		3.00		11	12
	.67	19	29					7		6.3		.90			
Tranquil	2.38	18	67							18.3	2.93			13	14
	.48	21	37							5.1	.62				
Somber	2.54	24	57		20			[	11]	19.6		2.72	8.36	11	14
	.80	19	33		15				11	6.1		.95	3.03		
Uneasy	4.40		52	22	[	81]	[	13]		26.1	3.36			11	9
	.90		44	35		16		14		4.7	.60				
Mournful	2.75	21			[	79]		[	6]		2.75	2.87	7.37	8	15
	.67	16				15			11		.83	1.17	3.03		
Melancholy	2.40	29							4	19.7	2.80		8.50	10	17
	.77	22							6	6	.75		3.18		
Delicate	2.88	19	71					[	17]	18.4	2.50			8	16
	.33	26	40						11	5.6	.50				
Triumphant	3.62			0	[	51]	[	37]			3.63		8.25	8	13
	1.00			0		16		20			.46		3.40		
Awkward					28			.8		28.6			8.00	5	20
					17			.8		5.1			3.17		
Glad	4.10	44		9				[	22]			3.50	10.40	10	17
	.94	43		24					17			.67	3.38		
Sentimental	2.62	17	70	13						19.7	3.00	3.75		8	18
	.66	24	39	26						6.4	.50	.43			

TABLE III (concluded)

SIGNIFICANT MEAN VALUES AND STANDARD DEVIATIONS FOR GROUPS OF PHRASES CHARACTERIZED BY A TERM

Terms	Tempo	Rhythms (percentages)			Intervals (percentages)					Orchestral range	Loudness	Mean pitce	Melodic range	Phrases	
		Smooth	Uneven	Rough	1st	2nd	3rd	4th	5th					Characterized	Not characterized
Whimsical	4.36							6	13			3.45	10.70	11	11
	.77							6	14			.63	2.46		
Flippant	4.00	42	20											5	19
	.89	28	17												
Exalted	2.73		57			50		14			3.44			11	10
	.87		36			18		13			.62				
Grotesque	3.86		43	33				[	11]	23.5		3.14		7	17
	.84		38	38					9	3.8		.64			
All pieces	3.32	30	52	18	16.67	53.03	15.02	7.85	7.43	22.8	3.22	3.33	9.90	40	
	.24	34	42	34	15.66	19.70	12.65	9.65	9.65	6.6	.76	.83	3.80		

The values in brackets under "intervals" are derived from the sums of the classes included between the brackets.
Tempo: Valued on a scale from 1 (slow) to 5 (fast)
Orchestral range: In terms of 8 tones to the octave
Loudness: Valued on a scale from 1 (soft) to 5 (loud)
Pitch: Numbers correspond to Octaves (2 below and 2 above middle C)
Melodic range: In term of 8 tones to the octave

TABLE IV

SIGNIFICANT DIFFERENCES, IN TERM OF STANDARD DEVIATIONS, BETWEEN AVERAGE VALUES FOR MUSICAL PHRASES CHARACTERIZED AND THOSE NOT CHARACTERIZED BY THE TERMS

Terms	Tempo	Rhythms			Intervals					Orchestral range	Loudness	Mean pitch	Melodic range	No. of pieces	
		smooth	uneven	rough	1st	2nd	3rd	4th	5th					Characterized	Not characterized
Animated	10.9	2.55	−2.52							2.16	2.04	1.56		11	13
Brilliant	11.0	3.26	−1.93						1.24		1.68		1.46	12	14
Dignified	−7.16	−2.46	3.70					1.31		−2.20		−2.29		11	12
Tranquil	−6.62	−1.90	1.64							−3.20	−2.30			13	14
Somber	−5.90	−1.93	2.20		1.17			[	−1.14]	−1.57		−2.47	−1.87	11	14
Uneasy	4.61		−1.35	1.00	[	2.53]	[	−2.50]		3.56	1.52			11	9
Mournful	−3.31	−1.54			[	2.19]		[	−3.39]		−1.81	−2.56	−2.53	8	15
Melancholy	−5.31	−1.25							−1.67	−1.49	−2.18		−1.44	10	17
Delicate	−3.48	−1.47	+1.68					[	1.17]	−3.20	−3.27			8	16
Triumphant	1.19			−1.20	[−3.25		]	[	2.46]		2.80		−1.32	8	13
Awkward					2.03			−3.65		2.94			−1.98	5	20
Glad	4.43	1.48		−1.01				[	1.52]			1.00	1.23	10	17
Sentimental	−2.61	−1.06	1.48	−1.02						−1.73	−2.27	2.31		8	18
Whimsical	3.44							−1.55	1.30			2.36	1.30	11	11
Flippant	3.39	1.51	−2.64											5	19
Exalted	−1.97		2.60			−1.00		1.98			1.53			11	10
Grotesque	2.15		−1.60	1.78				[	−1.74]	1.75		−1.07		7	17

The values in brackets under "intervals" are derived from the sums of the classes included between the brackets.

peculiarly individual traits of the selections. As a consequence, the number of selections used to represent a given term ranges from 5 for 'flippant' and 'awkward' and 7 for 'grotesque' up to 12 for 'brilliant' and 13 for 'tranquil.' Thus a total of 160 selections is used for determining the characteristics of the terms, and hence, each selection is used on the average in the determination of four different categories. The number of reports sufficient to get a selection into the favored group for a term likewise varied considerably. For 5 terms the lower limit of votes was more than 20. These include 'animated' and 'grotesque' (30 votes), 'uneasy' and 'mournful' (25 and 24 votes) and 'tranquil' (20 votes). Three moods had a lower limit that was less than 15 votes; 'awkward' (13), 'flippant' (12), and 'exalted' (9). These last three terms, it may be seen from Table II, are the three least used characterizations. In the case of votes for 'exalted,' for instance, there is only on selection (the 8th, the *Gregorian Chant*) which received more than 15 votes. It got 19. Although this may be the most exalted of the 40 selections, the character 'exalted' stands only fifth in the list of attributes of this selection, following 'dignified' (32 votes), 'somber' (31), 'mournful' (28), and 'melancholy' (21). It is plain that, when the same piece contributes to the determination of the musical characteristics of as many as 6 different categories, and when the possibility of a 10 or 15% vote is sufficient to get a selection classed as expressing a certain category, a fairly wide variety of pieces will be included under one term and the musical characteristics will not be any too clear-cut.

Table III presents the means and the standard deviations for all 40 selections in each of the musical factors but one used in this analysis. The analysis of the selections with regard to the kind of instrument carrying the melody does not seem amenable to this type of presentation. The table also includes the means and the standard deviations for songs grouped under each of the characterizations for those musical factors in which these values are somewhat significantly different from the means of the songs not characteristic of this term. There are 14 selections, for instance, that have none or only one vote to characterize these pieces as 'brilliant.' These negative, or contrast, groups range in number from 9 selections for 'uneasy' to 20 for 'awkward.' The significance of the data is better shown in Table IV, where, instead of presenting the values for the positive group of selections for each category, there is presented the differences between the means of the groups of selections characterized and not characterized by the category, in terms of the standard deviation of the difference.

The table is arranged so that the most significant factor analyzed occupies the extreme left-hand column of figures, and the others take their proper positions to the right. The location of the terms is approximately in order of the sharpness with which they can be distinguished. Speed is by far the most important factor in distinguishing the pieces, and is significant for every term except 'awkward.' The rhythms distinguish every category but 'awkward' and 'whimsical.' Range of the melodic line seems least significant, since it distinguishes only 8 terms. Twelve of the terms are distinguished in 6 to 8 of the musical factors, while 'flippant' and 'awkward' are distinguished only by 3 and 4 factors, respectively. There is a total of 99 items in this table of significant differences, of which 27 are between 2.00 and 2.99 sigma and 21 greater than 3 sigma.

The specific characteristics of the various categories may readily be seen by an inspection of these tables. A few special items deserve mention. Fast selections are

usually of smooth rhythm, and are loud. 'Exalted,' however, is alone typically loud and slow. Two categories are fast, but accentuate their apparent speed with an excess of rough rhythms, 'uneasy' and 'grotesque.' Again, 'brilliant,' which is always fast, has likewise an excess of fifths, and a consequent wide melodic range. Compare speed and range. While in general fast pieces have a wide range, and slow pieces a narrow range, 'triumphant' tends toward speed with narrow range. But despite the narrow range, 'triumphant' has an excess of thirds and fourths, and is deficient in firsts and seconds. Again, if we compare mean pitch and orchestral range, the general tendency is reversed by two terms; 'sentimental' is high, but with a narrow range, and 'grotesque' is low, but with a wider range.

The data with regard to the type of instrument carrying the melody was analyzed by comparing, for each characterization, the proportion of selections that used strings, wood-winds, brass, or piano. The significance is increased by comparing these percentages with the parallel ones for the sets of selections not characterizing each term. Differences between the two groups of selections for each category as large as 25% occur in 20 cases. The distribution of the categories especially using a certain instrumentation may conveniently be seen from the following. (The numbers after the terms correspond to the rank order of the second factoral loading, and will be considered in the next section.)

Brass		Wood-wind		Piano		Strings	
triumphant	5	mournful	3	delicate	17	glad	13
		awkward	1	tranquil	14		
grotesque	2	uneasy	6	sentimental	16		
				brilliant	10		

The categories especially not using a certain instrumentation follow:

melancholy	7	brilliant	10	mournful	3
tranquil	14	glad	13	awkward	1
delicate	17			triumphant	5
sentimental	16			somber	4

No piano selection of our forty phrases was classed as 'mournful,' 'triumphant,' or 'awkward,' and only one 'somber' selection used the piano. Here is an interesting contrast between 'brilliant' which uses the piano, and 'triumphant' which does not. Is this a difference significant of the types of quality of the instruments, or only characteristic of the selection of pieces here presented? Note that the terms expressed in brass or wood-wind are not represented in piano or strings, and vice versa.

None of the 4 remaining terms, 'animated,' 'exalted,' 'flippant,' or 'dignified,' has any difference that satisfies the criterion.

The matter of tone quality expressed by the various instruments is shown to be very significant in the determination of particular characterizations. Some experimental work using the same composition, but different orchestration, might reveal quite clearly the extent of our dependence for arousing mood on proper tone quality.

Although these and other differences can be detected by an inspection of the averages, or of the table of significant differences between the characteristics of the various groups of pieces, it is still quite obvious that very large overlapping exists within the members of the groups. We may compare, for instance, the musical

characteristics of the selections listed in Table I as belonging to some particular term with the mean values for all the pieces that together determined the characteristics for that term. The widest disparity occurs for categories 'brilliant,' 'somber,' and 'flippant,' where the pieces most typical of the category differ by a sigma or more from the average value of the groups in about one-third or more of the significant items.

Factors	Characterization of European musical phrases	Appropriate situations for	
		Indian songs	European folk-songs
Many Rough Rhythms	grotesque uneasy	after killing warrior scout song victory	victory war march
Many Uneven Rhythms	delicate sentimental dignified exalted somber	disappointment in love parting happy love	death of lover description of, or song to love
Few Uneven Rhythms	flippant animated grotesque brilliant	after killing warrior recitatives victory	victory
Many Smooth Rhythms	brilliant animated flippant glad	war medicine parting death of lover healing happy love	lonesome or sad gay or playful
Many 1sts and 2nds	uneasy mournful awkward	war medicine death of lover healing recitatives war path	sentimental or serious love lonesome or sad absence or parting
Many 3rds	triumphant	absence of lover after killing warrior war organization	victory
Many Large Intervals	glad exalted delicate	disappointment in love lonesome scout	gay or playful lonesome or sad death of lover war dirge

Another method of comparison is to see how many of the average values for the various terms would fit that category into the average (plus or minus one sigma) of the other terms. In this way, the average 'uneasy,' 'whimsical,' and 'flippant' phrase would fit into the middle range of 'glad' music. The middle range for 'exalted' will include the mean values for 'dignified,' 'tranquil,' 'somber,' 'awkward,' and 'sentimental.' The other categories are similar. How much of this ambiguity is due to the intrinsic difficulty of judging, and how much to the factors of presentation, such as the shortness of the phrases, the speed of their succession, and the unfamiliarity with such types of observation, can only be tested by further experimental work where these and other modifications in the experimental situation may be imposed.

A comparison of the musical characteristics typical of a certain category with the musical characteristics of analogous categories in folk music and

the music of American Indian songs is now possible. Only a rough comparison can be made, since the folk and Indian songs are not typified as to their character by the reports of any instructed group of *O*s. They are classified in terms of similarity of titles, or by the similarity of the situations to which they were appropriate. We cannot say that any of the seventeen categories used in this experiment may be equated to, say, organization war songs, recitatives, healing songs, or war marches. The kinds of characteristics typical of our music have only a partial overlapping with the character indicated for the situations that call for song by the comparison groups. The only data from the folk songs includes the rhythms and three classes of intervals. We have for the Indian songs data also on pitch, range, and speed. Finally, a comparison must neglect the actual values and percentages found, since the data include, on the one hand, instrumental music and, on the other, songs.

Despite these difficulties there seems to be a close approximation in effect of similar music upon different peoples, as may be seen from this comparison of certain factors in European music with selected folk songs and American Indian songs.

Notice especially with regard to rhythms the relation of 'grotesque' to the 'victory' songs and those sung 'after killing a warrior.' These also have few large intervals. 'Grotesque' parallels the 'general war songs' and the 'war organization' songs, too, in being fast, low in pitch, and wide in (orchestral) range.

High Pitch	European	sentimental, whimsical, animated, glad
	Indian	happy love, recitative
Low Pitch	European	mournful, somber, tranquil, dignified, grotesque
	Indian	general war, organization of war party
Wide Range	European	uneasy, animated, grotesque, brilliant, glad
	Indian	general war, organization for war party.
Narrow Range	European	tranquil, dignified, delicate, mournful, awkward, somber
	Indian	healing, scout, war path
Fast	European	brilliant, animated, uneasy, glad whimsical, flippant, grotesque
	Indian	general war, organization for war
Slow	European	dignified, somber, tranquil, melancholy, mournful, delicate, sentimental
	Indian	in battle, sad love

These general similarities strengthen the presumption that the language of music uses conventions that are not purely arbitrary.

(3) The interrelations of the descriptive terms. Rank-difference correlations between the votes on the different pieces for each pair of terms were determined. These together with the reliability coefficients are in Table V.

TABLE V

INTERCORRELATIONS BETWEEN THE DESCRIPTIVE TERMS

Terms	Mournful	Awkward	Uneasy	Somber	Delicate	Dignified	Flippant	Exalted	Melancholy	Grotesque	Triumphant	Sentimental	Whimsical	Animated	Glad	Brilliant	Tranquil
Mournful	.86	.40	.19	.76	−.03	.38	−.40	.21	.78	.16	−.04	.12	−.42	−.46	−.60	−.46	.24
Awkward	.40	.77	.39	.27	−.43	.04	−.03	−.06	.28	.69	.14	−.22	−.10	−.03	−.31	−.10	−.27
Uneasy	.19	.39	.88	−.12	−.44	−.41	.23	−.20	−.13	.56	.32	−.46	.20	.58	−.03	.36	−.57
Somber	.76	.27	−.12	.89	.09	.77	−.65	.45	.89	−.12	−.11	.25	−.53	−.69	−.66	−.65	.50
Delicate	−.03	−.43	−.44	.09	.85	.22	−.05	.01	.22	−.60	−.47	.84	.21	−.35	.08	−.42	.78
Dignified	.38	.04	−.41	.77	.22	.90	−.51	.59	.61	−.28	.01	.25	−.45	−.59	−.37	−.53	.52
Flippant	−.40	−.03	.23	−.65	−.05	−.51	.77	−.26	−.49	.28	.23	−.27	.45	.68	.63	.60	−.52
Exalted	.21	−.06	−.20	.45	.01	.59	−.26	.54	.26	−.40	.17	.09	−.44	−.22	−.06	−.20	.25
Melancholy	.78	.28	−.13	.89	.22	.61	−.49	.26	.88	−.19	−.37	.34	−.43	−.72	−.63	−.68	.61
Grotesque	.16	.69	.56	−.12	−.60	−.28	.28	−.40	−.19	.86	.34	−.56	.11	.32	−.08	.28	−.55
Triumphant	−.04	.14	.32	−.11	−.47	.01	.23	.17	−.37	.34	.78	−.55	−.15	.45	.20	.46	−.47
Sentimental	.12	−.22	−.46	.25	.84	.25	−.27	.09	.34	−.56	−.55	.78	.13	−.56	−.22	−.57	.83
Whimsical	−.42	−.10	.20	−.53	.21	−.45	.45	−.44	−.43	.11	−.15	.13	.82	.41	.51	.31	−.18
Animated	−.46	−.03	.58	−.69	−.35	−.59	.68	−.22	−.72	.32	.45	−.56	.41	.92	.57	.88	−.66
Glad	−.60	−.31	−.03	−.66	.08	−.37	.63	−.06	−.63	−.08	.20	−.22	.51	.57	.84	.61	−.37
Brilliant	−.46	−.10	.36	−.65	−.42	−.53	.60	−.20	−.68	.28	.46	−.57	.31	.88	.61	.85	−.76
Tranquil	.24	−.27	−.57	.50	.78	.52	−.52	.25	.51	−.55	−.47	.83	−.18	−.66	−.37	−.76	.94

The correlations range from +0.89 for 'somber' and 'melancholy,' to —0.76 for 'brilliant' and 'tranquil.' Eight or nine of the correlations are about as high as, or higher than, the reliability coefficients. A closer examination of groups of terms shows some interesting relationships. The three categories 'mournful,' 'somber,' and 'melancholy' have an average inter-correlation of 0.81. Similarly, the categories 'delicate,' 'sentimental' and 'tranquil' intercorrelate 0.816. Almost half of these selections are piano, and only one (Herbert's *Indian Summer*) uses brass. The set 'somber,' 'dignified,' and 'tranquil' intercorrelate 0.60. At the other end of the scale are the four moods 'animated,' 'flippant,' 'brilliant,' and 'glad' which have an

TABLE VI

FACTOR LOADINGS DETERMINING THE INTERRELATION OF THE DESCRIPTIVE TERMS

Terms	1st factor	2nd factor	3rd factor	4th factor
Animated	.895	.027	.142	—.351
Brilliant	.868	.069	.226	—.187
Flippant	.692	—.197	—.054	—.061
Glad	.616	—.398	.342	—.004
Whimsical	.454	—.470	—.307	—.082
Uneasy	.451	.439	—.340	—.494
Grotesque	.428	.574	—.447	.288
Triumphant	.406	.445	.374	—.012
Awkward	—.005	.601	—.428	—.163
Exalted	—.397	.191	.590	—.118
Delicate	—.454	—.752	—.030	—.155
Mournful	—.562	.525	—.268	—.286
Sentimental	—.635	—.627	—.198	—.159
Dignified	—.723	.222	.403	.041
Tranquil	—.821	—.460	.023	—.032
Somber	—.834	.470	.023	—.152
Melancholy	—.836	.299	—.200	—.086

average intercorrelation of 0.66. These four moods average a —0.48 with 'mournful,' —0.50 with 'dignified,' —0.58 with 'tranquil,' —0.63 with 'melancholy,' and —0.66 with 'somber.' The groupings do not, I think, indicate an identity between the various categories as a comparison with the significant items distinguishing the terms, shown in Table IV, indicates. There can be little doubt that a number of different factors are at work in the determination of these intercorrelations.

A method for determining the weighting and the number of these factors is provided by Thurstone's simplified multiple-factor technique.[5] Four different factor loadings have been computed, and are listed in Table VI. The first factor has a loading of from approximately 0.4 to 0.9 for every term except 'awkward.' There are only 4 loadings in the third factor as high as 0.4, and only one in the fourth factor.

Since the very first discussion of the use of factor analysis, the caution has been urged never to name the factors. For, it is said, such a naming is worse even than joining the Faculty psychologists, since the error of their

[5] L. L. Thurstone, *A Simplified Multiple Factor Method*, 1933, 1-25.

ways has been so often pointed out. Despite these warnings, however, I do not hesitate to suggest names at least for three of these four factors. In this study the characteristics of the music have in part been analyzed, and it is possible to determine which of the musical features is related to the various factor loadings. The first and most important factor is the dynamical phase of the music. This factor correlates 0.946 with the rank order of the tempo items listed in Table III. It also correlates about 0.92 with smooth rhythms, and about 0.66 with loudness. The second factor is a little ambiguous. It correlates between 0.45 and 0.65 negatively with melodic range, pitch, and a combination of thirds, fourths, and fifths, and positively with firsts and seconds, and with orchestral range. Roughly combining the ranks for these 5 items, and correlating the consequent order with that for factor 2 gives a figure of 0.88. This factor might be called the tonality characteristic of the selections. This suggestion becomes more plausible when we consider the factoral loadings in relation to the types of instrument used to carry the melody. The numbers after the categories listed in the consideration of the orchestrations are the rank orders of the second factor loading (see p. 636). The third factor most definitely is related to the extent of the large intervals. The significance of an excess of thirds, fourths, or fifths correlates with this factor 0.92. This factor also correlates with rough rhythms about 0.60. Should we call this factor in contrast to the dynamics of the first factor, and the tonality placing of the second, the factor of motility? The fourth factor seems directly unrelated to any single analyzed item. It is plain to see that the factor is relatively small, and that all the analyzed musical items have been used; hence this factor must relate to some complication of several factors, or it must be uncovered by an analysis of factors not included in this program.

Another method is considering the similarity and opposition of the various categories is to plot them in terms of the factoral values. Consider such a plot for the first two factors for the 17 terms.

Grouped together with positive ratings in both factors stand the terms 'uneasy,' 'triumphant,' and 'grotesque.' In the opposite quadrant to these, but more scattered, stand the three terms with negative weights in both factors; 'delicate,' 'sentimental,' and 'tranquil.' Comparable to these last in factor 2, but opposed to having high weightings in dynamical aspect 1, are 'whimsical,' 'glad,' and 'flippant.' The remaining quadrant includes the five high in tonality and low in dynamics; 'exalted,' 'dignified,' 'melancholy,' 'mournful,' and 'somber.' The remaining three terms have zero values in one or the other factor: 'animated' and 'brilliant' have the highest weightings in dynamics, but practically none in factor 2, while 'awkward' has the highest weighting in the second factor, and none in the first. The entire distribution forms a sort of hollow circle, in which only the term 'exalted' falls noticeably within the circumference, and the greatest gap in the circle lies between 'delicate' and 'whimsical.'

A plot of the third and fourth factors results in a triangular figure with the apex high in the third factor but practically zero in the fourth. 'Exalted' stands here at the top. 'Triumphant,' 'glad,' and 'dignified,' each form a different quadrant in the preceding plot of factors, here cluster about a common point below 'exalted,' with medium weighting in factor 3, and zero in factor 4. A second corner of the triangle is made by 'uneasy' and next to it lies 'mournful.' 'Animated' stands halfway between 'exalted' and 'uneasy.' The third corner finds 'awkward' and 'grotesque.' A point between 'grotesque' and 'uneasy' is occupied by 'whimsical' and a point between 'grotesque' and 'exalted' is occupied by 'somber.'

If some pictorial 4 dimensional diagram of the relations between these categories could be presented, terms that are approximately similar could be identified, and the location of lacunae in the classification could be discovered and remedied.

Summary and Conclusions

The problem as to the nature of the musical factors that are responsible for the mood-characteristics of music was attacked on two fronts; first, by analyzing the reports of more than 100 *O*s upon the characteristics expressed by 40 musical phrases selected primarily from orchestral and instrumental pieces of music, and, secondly, by establishing a certain dependence of the selections expressing a common quality upon the underlying similarities within the composition in regard to speed, loudness, orchestration, rhythm, intervals, range, pitch, and other factors.

Although individuals may vary to a considerable extent as to how they will characterize a selection, the reports of the three groups of *O*s combined provide very satisfactory data for a preliminary analysis. It appears quite certain that many pieces of good music can elicit in many auditors a fairly uniform characterization solely through factors resident within the musical structure.

The paper presents an analysis of a number of musical factors that show a considerable relation to the quality expressed by the music. The interrelation between the various categories used, and the factoral basis for such interrelations have been shown. A comparison of these findings with a previous analysis of the songs of Indians and of Folk groups shows that similar musical elements, in terms of rhythms, frequency of various intervals, and other items, result for these divergent groups in comparable effects upon the hearer.

The particular factors within the musical structure that are here found to be responsible for the mood-determination may prove on further analysis to be either incomplete or improperly weighted. Since the range of selections was limited, since the categories were limited, and since the weightings were worked out in terms of this particular set of reports, we can not be

sure how close the numerical summaries approach generalizations holding for other types of music. Three problems involving the same categories and a comparable musical analysis immediately present themselves. The first involves a selecting of phrases from good music that fit into the categories of the various moods, and a testing of this prediction with the reports of a group of *O*s. The second problem involves an extension of the method to songs. The third, which is now under way, consists in the artificial construction of 34 selections, two of which have the properties of each of the 17 categories herein described and used, one of them designed to express the given quality and the other designed not to express it. If it is possible to construct phrases that conform to the pattern for a given category, but are adjudged by auditors to typify widely divergent terms, then the present analysis is certainly at fault; but the means for its correction are also certainly at hand.

THE PHYSIOLOGICAL BASIS OF THE PERIODIC MERGING OF AREA INTO BACKGROUND

By G. A. Fry[1] and V. M. Robertson,[2] Washington University

When an area of a certain brightness on a slightly darker background is presented to the eye, the area is seen to merge periodically into its background. The early investigators of this phenomenon attributed it to the fluctuation of attention which periodically reënforces the excitations from the eye. There remained, however, the possibility that the impression is periodically obliterated at the peripheral level. Ferree in particular has espoused this view.[3] He proposed that the area disappears because of fatigue of the photoreceptors and reappears because eye-movements relieve this fatigue. When the field surrounding the area is dark, it is easy to see that the photoreceptors in the stimulated area might become less and less sensitive to the light until they would reach a resting stage comparable to that of the photoreceptors outside the area of stimulation. When, however, the surrounding parts receive some light, the problem is more complicated. The phenomenal disappearance of the area could, nevertheless, be brought about by a differential rate of fatigue; that is to say, the intensely stimulated photoreceptors might fatigue more rapidly than the feebly stimulated ones, so that both finally would approach the same level of activity.

It seems to us that Ferree has carried his explanation too far toward the periphery of the visual mechanism, for certain facts indicate that the impression is not obliterated until it reaches the retinal synapses. In a previous paper,[4] evidence was presented for a special process of interaction between the retino-cortical paths in the region of a border between a darker and a brighter area. Certain facts discussed below indicate that the discrimination of an area from a slightly darker background depends essentially upon the

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[3] C. E. Ferree, An experimental examination of the phenomena usually attributed to fluctuation of attention, this Journal, 17, 1906, 81-120.

[4] G. A. Fry, The relation of border-contrast to the distinctness of vision, *Psychol. Rev.*, 38, 1931, 542-549. Helmholtz, Hering, and others have pointed out the relation of border-contrast to the distinctness of vision, but it is not clear that any of them has based his theory of border-contrast upon the interaction of the retino-cortical paths.

establishment of this contrast-process at the border. What happens when the area disappears is that the border-process fatigues and breaks down and the area assimilates the brightness of the surroundings. When the border-mechanism has sufficiently recovered, the impression of the area becomes reëstablished. The fatigue of the photoreceptors might contribute to the disappearance of the area, but only in an indirect way, *i.e.* only in so far as they render the impression at the photoreceptor level so faint that the synaptic border process can no longer maintain itself.

Although Guilford has shown that reappearances tend to be preceded by eye-movements,[5] it is quite conceivable that eye-movements help to produce reappearances by directly affecting the synaptic processes rather than by relieving fatigue of the photoreceptors.[6] If the reappearance of a spot depended solely upon the sudden relief of photoreceptor fatigue by an eye-movement, the time required for the spot to reappear should be determined by the chance occurrence of an eye-movement. This hypothesis is contradicted by the fact that the time of reappearance varies with such stimulus-factors as size of area, brightness-difference, etc., which cannot affect the frequency of eye-movements. On the other hand, if the reappearance depended primarily upon the recovery of synapses from fatigue, the speed of recovery and the ease of the final establishment of the border-contrast process might depend upon such factors as brightness-difference, size of area, etc. An eye-movement, by directly affecting the synapses, might facilitate the final establishment of the process, but in so doing would make the reappearance occur only slightly sooner than if the synaptic process had to establish itself unaided.

Although reappearances tend to be preceded by eye-movements, this does not rule out other factors as partial determiners of the time of reappearance. The fact that not all eye-movements produce reappearances indicates that an eye-movement merely facilitates reappearance and does not produce a reappearance unless the spot is about ready to reappear for other reasons. The average time of reappearance could be largely determined by brightness-difference, size of area, etc., eye-movements determining in what direction and to what extent an individual reappearance deviates from the average time required. On this basis each individual reappearance would tend to be preceded by an eye-movement regardless of when it comes with respect to the average time of reappearance.

Whereas Guilford found that eye-movements occur most frequently just prior to reappearances, he also found that they occur least frequently just prior to disappearances. The implication of the finding is that eye-movements not only facilitate reappearances but also prevent disappearances. This fact needs to be taken into consideration also before a complete account of the effect of eye-movements can be rendered.

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[5] J. P. Guilford, 'Fluctuation of attention' with weak visual stimuli, this JOURNAL, 38, 1927, 534-583.

[6] An eye-movement has at least three immediate effects. (1) It causes the image to be received by a different part of the retina. (2) A mechanical force is set up by the rotation of the eye in its orb. (3) The intraocular pressure is changed. Any one or more of these factors might affect processes at the retinal synapses, but we do not propose to delve into this problem in the present paper.

From an interest in eye-movements Ferree investigated the effect of varying the stimulus-area. He found that, when the area is large, it may be seen at the onset of stimulation but once it disappears it never reappears. When the size is somewhat smaller the area appears for short periods between long intervals, and as the size is further decreased the visibility periods lengthen and the intervals between shorten until the area is seen continu-

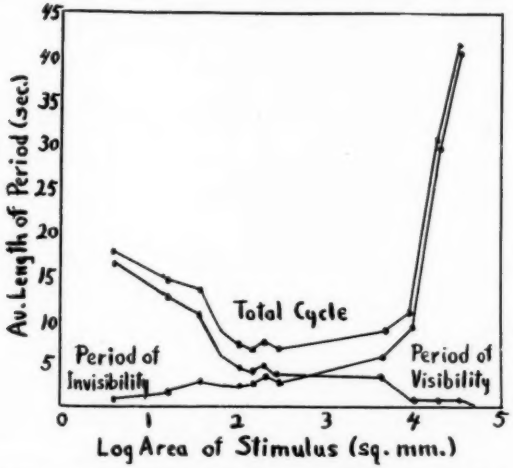

FIG. 1. SHOWING THE EFFECT OF VARYING THE SIZE OF THE STIMULUS-AREA UPON THE LENGTH OF THE VISIBILITY AND THE INVISIBILITY PHASES AND THE TOTAL CYCLE

The total cycle is the sum of its two phases. The length of the total cycle is the reciprocal of the rate of reappearances. The graph is based upon Ferree's data.

ously. Fig. 1 shows such results obtained with square areas of different sizes observed at a distance of 1 m.[7] The average visibility period decreases and the average invisibility period increases as the area is increased. Another feature of the results, which has been emphasized by Ferree, is that the duration of the total cycle decreases to a minimum and then increases, or, in other words, the frequency of fluctuations increases to a maximum and then decreases. He alleged that the frequency of reappearances depends upon the frequency of eye-movements, and that with a small area fixation is steady, with a larger area the eye is tempted to move to see the edges, but with a still larger area the edges no longer attract attention and the eye is not tempted to move from the point of fixation.

[7] C. E. Ferree, *op. cit.*, 102, Table VIII.

As already pointed out, eye-movements both prolong the visibility phases and also shorten the invisibility phases, but they do not necessarily change the length of the total cycle. Therefore, if it correlates with anything, the frequency of eye-movements should correlate with the visibility-invisibility ratio, but as the area is increased this ratio gradually decreases and does not first rise to a maximum and then decrease corresponding to Ferree's theory. Since the visibility and the invisibility phases both vary in a definite way with change in area, and since the total cycle is merely the sum of its two phases, its variability may be regarded as a mathematical function of the variability of its two phases, and no further significance needs to be attributed to the change in the rate of fluctuations.

The fact that the border-process at the retinal synapses plays an essential rôle in the fluctuation phenomenon is proved first of all by the fact that the times required for the area to disappear and to reappear are affected by the blurredness of the border which cannot affect the fluctuations through fatigue of the photoreceptors, but which might be conceived to affect the border-process.

EXPERIMENT I

In the first experiment we sought to determine the effect of the sharpness or definiteness of the retinal image upon the fluctuations of a stimulus-spot of light.

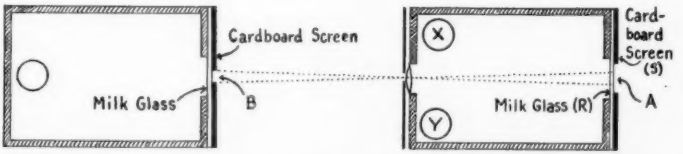

FIG. 2. APPARATUS FOR DEMONSTRATING THE RECURRENT VISIBILITY OF A SPOT
A disk-shaped area (A) of the milk-glass screen (R) is illuminated by light sources X and Y. A spot at the center of this area receives an additional illumination from B. This spot can be seen to merge periodically into its background.

The apparatus illustrated in Fig. 2 was used. The circular aperture (1½ in. in diam.) in the black cardboard screen (S) exposed a disk-shaped area (A) of the milk glass screen (R), which was illuminated from behind by light sources, X and Y. The intensity of this illumination could be varied by rheostat but was kept constant at such a value that the brightness of A was 0.025 c. per sq.ft. The lens projected an image of B upon the milk-glass screen at the center of A. The brightness of this central spot could be varied by changing the size of the aperture at the lens, but was kept constant so that the spot was 0.025 c. per sq.ft. brighter than the rest of A. This brightness relationship permitted phenomenal fluctuations satisfactory for our

purpose. The left eye was shielded. The right eye was paralyzed with homatropine, and used in the experiment. The head was held steady in a head-rest so that the right eye was 1 m. away from the stimulus. A point at the center of the spot was fixated. Lenses having refracting powers from —2 to +4 diopters were placed in front of the eye. The results of F (Fry) are shown in Fig. 3. The values represent averages taken from a 1-min. record for each lens.

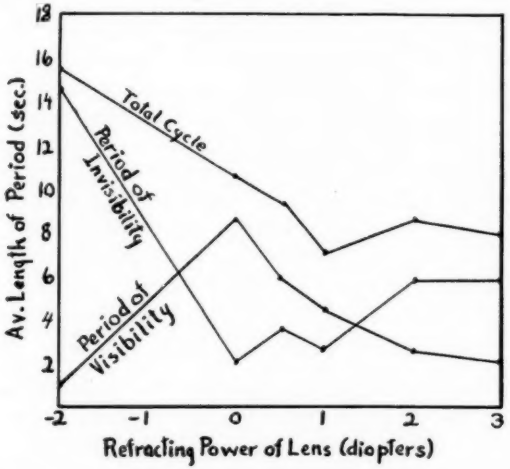

FIG. 3. SHOWING THE EFFECTS OF VARYING THE SHARPNESS OF THE RETINAL IMAGE UPON THE LENGTH OF THE VISIBILITY AND INVISIBILITY PHASES

The results show that when the retinal image is thrown out of focus in either direction, the visibility phases shorten and the invisibility phases lengthen. In the case of the + 4 lens the spot was invisible during the entire minute.

In a previous paper,[8] it was pointed out that owing to the defects in the optical system of the eye the image upon the retina is blurred and that sharp vision is possible only by virtue of the special physiological processes underlying border-contrast which create a border where none exists in the physical stimulus. In connection with the present problem it is proposed that the change in the visibility-invisibility ratio with degree of blurredness of the optical image merely measures the difficulty which the border-contrast process meets in overcoming blur.

Since the whole spot assimilates the brightness of the background whenever the contrast border breaks down, this assimilation must be supposed

[8] G. A. Fry, *op. cit.*, 542-549.

to involve interaction of the retino-cortical paths, for a change occurring at the edge of an area cannot be supposed to affect the photoreceptors at the center.

The following experiment supplements the one just described because it demonstrates how one can increase or decrease the rate of change in the recurrent visibility of an object by voluntary changes in attention, which change accommodation and thus affect the sharpness of the retinal image.

EXPERIMENT II

The object of the second experiment was to determine the effect of voluntary changes in attention upon the recurrent visibility of a stimulus-object.

The same stimulus was used in this experiment as in Experiment I. The left eye was shielded and the right eye used. The experiment was performed once with the right eye normal and again with the right eye paralyzed with homatropine. A —2 lens was used in connection with the normal eye so that considerable accommodation was required to focus the stimulus. Relaxing accommodation threw the stimulus out of focus. When the eye was paralyzed the lens best suited for clear vision was used, +2 for *R* (Robertson) and +0.5 for *F*. An artificial pupil 2 mm. in diam. was used in all cases to render ineffective the changes in the size of the pupil. The pupil of the paralyzed eye is constant in size but much larger than that of the normal eye. Then, too, the pupil of the normal eye is subject to fluctuation even though the stimulus-conditions are kept constant.[9] In one experiment *S* attempted to accelerate the rate of fluctuation by hastening both the disappearances and also the reappearances, and in the next experiment he tried to retard the rate by delaying both reappearances and disappearances. The attempt to hasten a reappearance or to delay a disappearance consisted in paying close attention to the stimulus, whereas the attempt to delay a reappearance or to hasten a disappearance consisted in observing the stimulus passively, merely noting whether the spot was present or absent. Voluntary eye-movements were not resorted to in order to produce the effects desired, for an attempt was made to fixate steadily a point at the center of the spot. Four minutes were allowed at the beginning of each experiment for dark adaptation and 2-min. intervals between the observation periods. *S* made a record of the fluctuations on a kymograph drum by pressing an electric key.

The results of the two *S*s are shown in Table I. The values represent averages of three 1-min. periods of observation. When the eye is normal, voluntary changes in attention are shown to change the frequency of reappearances, but this effect disappears when the eye is paralyzed. The explanation of this finding seems to be that certain effects produced by changing attention are brought about by changes in accommodation.

One weakness of Experiment II, in so far as it aimed to demonstrate the

[9] Troland has shown that fluctuations in the size of the natural pupil can materially affect the rate of fluctuations under normal conditions. (L. T. Troland, The colors produced by equilibrium adaptation photopic adaptation, *J. Exper. Psychol.*, 4, 1921, 362-390.)

effect of changes in the sharpness of the retinal image, is that changes in accommodation produce changes not only in the sharpness of the retinal image but also in the intraocular pressure, and this latter factor might contribute to the results obtained. The following experiment demonstrates that this factor does play a rôle in a fluctuation of an after-image.

TABLE I

EFFECT OF VOLUNTARY CHANGES IN ATTENTION UPON THE RECURRENT VISIBILITY OF A STIMULUS-OBJECT

S	Attitude	Condition of eye: Normal — Av. vis. period	Normal — Av. inv. period	Normal — Av. total cycle	Paralyzed — Av. vis. period	Paralyzed — Av. inv. period	Paralyzed — Av. total cycle
R	Retard	21.70	1.60	23.30	11.62	2.86	14.48
	Accelerate	3.23	2.00	5.23	10.53	2.46	12.99
F	Retard	10.40	7.44	17.84	6.32	3.80	10.12
	Accelerate	3.93	2.31	6.24	6.69	3.44	10.13

EXPERIMENT III

The object of the third experiment was similar to that of the second. We here sought to determine the effect of voluntary changes in attention upon the fluctuation of an after-image.

The left eye was shielded and the right eye fixated a bright disk in a dark field for 30 sec. *S* then closed his eye and the bright disk was exchanged for a dimly illuminated field. The change required about 5 sec. *S* again opened his eye and observed the negative after-image. The brightness of the disk was 13 c. per sq.ft. It was 4 cm. in diam. and 52 cm. from the eye. The dimly illuminated field was 5 in. by 7 in. in size and its brightness was 0.62 c. per sq.ft. A fixation-point was used both at the center of the disk and at the center of the dim field.

The experiment was performed once with the eye normal and again with the eye paralyzed. Robertson served as *S* and used a +4 lens when his eye was paralyzed. An artificial pupil 2 mm. in diam. was used to render ineffective the changes in the size of the natural pupil. The head was held steady in a head-rest.

An attempt was made in alternate periods to decrease and increase the rate of fluctuations. Four minutes of rest were allowed at the beginning of each trial and 2-min. intervals between periods.

The results are shown in Table II. The values represent averages for three observation-periods. The after-image lasted from 40 to 60 sec., but only the first 30 sec. are represented in our measurements. When the eye is normal, voluntary changes in attention change the rate of fluctuations, but fail to do so when the eye is paralyzed. The explanation of this effect seems

to be that it is brought about by changes in the intraocular pressure, because changes in the sharpness of the retinal image were eliminated by the use of an after-image. We have no theory to offer as to how changes in intraocular pressure can affect the fluctuation of an after-image.

Another fact which proves that the phenomenal merging of an area into its background is a function of interaction of the retino-cortical pathways is that, under certain conditions, borders in different parts of the field of

TABLE II

EFFECT OF VOLUNTARY CHANGES IN ATTENTION UPON THE FLUCTUATION OF AN AFTER-IMAGE

Attitude	Condition of eye					
	Normal			Paralyzed		
	Av. vis. period	Av. inv. period	Av. total cycle	Av. vis. period	Av. inv. period	Av. total cycle
Retard	12.30	9.00	21.30	11.43	3.40	14.83
Accelerate	4.36	3.23	7.59	11.37	3.16	14.53

vision break down and build up in alternate fashion. An instance of this can be demonstrated with a pattern like Fig. 4 A which consists of 9 white squares on a dark ground. When a point at the center is fixated for a while and the eyes are then closed and shaded, the negative after-image will fill in so as to appear at one time as a group of three horizontal bars (Fig. 4 B) and at another time as a group of vertical bars (Fig. 4 C). If all the borders broke down and built up simultaneously, so that one would

FIG. 4. ILLUSTRATING HOW BORDERS IN DIFFERENT PARTS OF THE FIELD MAY BUILD UP AND BREAK DOWN IN ALTERNATE FASHION

An after-image of 9 small squares like A is seen alternately as three horizontal bars (as in B) and as three vertical bars (as in C).

see first a large solid square and then a group of 9 small squares, the simultaneous breaking down of the borders and the filling in of the gaps could be attributed to a uniform rate of adaptation of the photoreceptors in the different parts of the retina, and the simultaneous building up of the borders could be attributed to eye-movements which affect all parts of the eye

simultaneously. But the fact that borders in different parts of the field break down and build up alternately can be attributed only to a sort of rivalry which involves the interaction of the retino-cortical paths.

The remainder of this paper deals with the relationships obtaining between the two eyes. If fluctuations were the same for binocular as for monocular vision, this fact could be explained in two ways; (1) by assuming that the two eyes yield steady impressions and that the fluctuation is due to a central factor like the periodic reinforcement of 'sensation' by attention, or (2) by assuming that the course of fluctuation is determined by

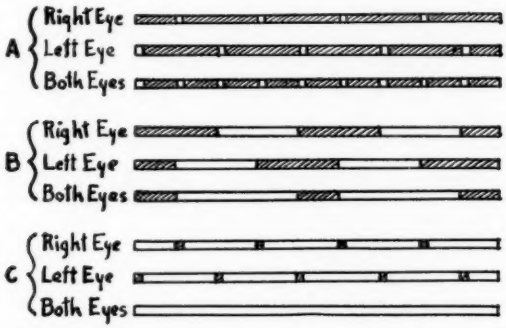

FIG. 5. SHOWING HOW THE BINOCULAR RECORD OF FLUCTUATIONS REPRESENTS A COMBINATION OF THE EFFECTS IN THE TWO EYES
The light and dark phases represent visibility and invisibility phases respectively.

peripheral factors which are synchronized for the two eyes, such as eye-winks, refixations, and changes in accommodation. Fortunately, however, both attempts at explanation can be dispensed with, for the experimental results described below show clearly that the impression delivered by each eye is not steady but intermittent, and that the rhythms for the two eyes are not synchronized. In other words, the impressions from the two eyes blunder upon each other at the common cerebral pathways without any preëstablished harmony. There is no reason in the first place for supposing that eye-movements should synchronize fluctuations for the two eyes. Not all movements produce reappearances, and there is no reason to suppose that an eye-movement which produces a reappearance in one eye would produce one also in the other. Changes in accommodation might be more effective in synchronizing the rhythms of the two eyes, but the fact that synchronization does not occur shows that this factor does not play an appreciable rôle under ordinary conditions.

Our interpretation of the results obtained with two eyes is based upon the assumption that an impression reaches the brain whenever one eye or the other transmits an excitation, or when both eyes transmit simultaneously. (1) When the visibility phases are short as compared with the invisibility phases, the visibility phases for the two eyes seldom overlap or coincide, so that the total effect for the two eyes is a doubling of the frequency of visibility periods, as indicated in A of Fig. 5. (2) When the visibility and invisibility phases are about equal for each eye taken separately, the visibility phases for the two eyes overlap and are spliced together so that the binocular-visibility phases are longer than the separate monocular phases, but there is no increase in frequency. This relationship is illustrated in Fig. 5 B. (3) When the visibility phases for one eye are very brief as compared with the invisibility phases, the invisibility phases for the two eyes seldom coincide so that an invisibility phase in one eye is always compensated by visibility in the other, and the cortex thus continuously receives an impulse from one eye or the other (see Fig. 5 C). That these effects do occur has been demonstrated by the following experiments.

EXPERIMENT IV

The object of the fourth experiment was to compare the fluctuations obtained with binocular and monocular vision.

The stimulus was provided by the apparatus illustrated in Fig. 2. Two different brightness relationships of the spot and ground were used. One-minute records of fluctuations were made for the right eye, for the left eye, and for binocular vision, in the order stated. Two-minute intervals were given between records. The eyes were 40 cm. from the stimulus. An artificial pupil (2.33 mm. in diam.) was used before each eye.

TABLE III

COMPARISON OF MONOCULAR WITH BINOCULAR FLUCTUATIONS

Brightness (c. per sq. ft.)		Total vis. per min. (in sec.)			Av. no reappearances per min.		
Ground	Spot	R	L	B	R	L	B
.034	.040	14	10	27	5	4	8.6
.034	.042	45	46	60	5	5	0

The results are shown in Table III. The values given for "total visibility per min." and for "number of reappearances per min." represent averages from two records in the case of the spot-brightness of 0.040 c. per sq. ft. But in the case of the spot-brightness of 0.042 c. per sq. ft. they represent averages taken from single records. This experiment demonstrates that when the visibility-invisibility ratio for the separate eyes is low, the frequency of

reappearances is twice as high for binocular vision as for monocular vision, and when the visibility-invisibility ratio is high for the separate eyes, the spot is seen continuously with binocular vision. In accordance with the principles set down above, these findings indicate that the fluctuations for the two eyes are not synchronized.

The fact that the binocular record is not a double-traced monocular record makes it essential to use monocular vision when an attempt is made to study the factors influencing the breaking down and building up of the border-contrast process in the retina.

Guilford[10] has investigated the rôle played by central fatigue in preventing a spot from reappearing. He found that, if, at the moment of disappearance for one eye, the stimulus was shifted to the corresponding area of the other eye, the spot reappeared almost immediately whereas it took two to ten times longer if it continued to act on the original eye. This fact seems to indicate that the important seat of fatigue is in the retina of the stimulated eye. But he found that it takes longer for the area to reappear when the stimulus is shifted to the corresponding area of the opposite retina than when it is shifted to a fresh area of the same retina, and he argued from this finding that a slight amount of central fatigue exists.

The notion of central fatigue is quite compatible with the general theory proposed above. A border-contrast process similar to that in the retina must be present at every synaptic juncture in the retino-cortical tract. It is quite conceivable, therefore, that fatigue in the central paths due to stimulation of one eye might prevent the central border-contrast process from being established even though the stimulus is shifted to the opposite eye.

Guilford also found that, if he allowed a little time to elapse after the disappearance of the spot in one eye before shifting the stimulus to the opposite eye, the time required for a reappearance was longer than when he withdrew the stimulus from the original eye at the moment of disappearance instead of letting it continue to act until the onset of stimulation in the other eye. This must mean that even though the border-contrast process breaks down in the first eye, the discharge in the optic nerve, though not capable of creating an impression of a spot, can, nevertheless, maintain the central paths in a state of fatigue.

Summary

The theory has been proposed that the essential condition for the periodic merging of a visually apprehended area into its background is the building up and breaking down of the process at the retinal synapses which subserves border-contrast, rather than the fatigue of the photoreceptors and the relief of fatigue by eye-movements.

The proof of this proposition has been based upon two facts. (1) The periodic merging of an area is affected by varying stimulus factors, such

[10] J. P. Guilford, *op. cit.*, 565-577.

as the blurredness at the border, which might be supposed to affect the interaction of the retino-cortical paths at the retinal synapses, but which cannot produce their effects by interfering with the fatigue-process of the photoreceptors. (2) Under certain conditions borders in different parts of the field build up and break down alternately. This effect can only be attributed to a sort of rivalry which involves the interaction of the retino-cortical paths. The fact that the essential cause of the mergings is peripheral rather than central is shown by the fact that the fluctuations of the two eyes do not occur at the same time.

Guilford's discovery that central fatigue can slightly delay the reappearance of an area has been shown to be compatible with the general theory proposed above.[11]

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[11] We are indebted to Professor P. W. Cobb, upon whom we have constantly relied for advice.

RELATION OF FORGETTING TO EXPERIMENTAL EXTINCTION

By GLEN FINCH and ELMER CULLER, University of Illinois

In any case of conditioning, repetition of the secondary stimulus without reënforcement by the primary leads notoriously to rapid extinction; whereas repetition of a learned verbal or manual response normally leads to fixation and improved retention. In one case, the probability that a given substitute response will be evoked by future applications of the secondary stimulus rapidly decreases, in the other markedly increases. The law of frequency, use, or repetition, which has long been treated as one of the characteristic principles of learning, seems to be directly opposed by the phenomena of experimental extinction. This one fact has induced some of our leading theorists sharply to differentiate conditioning from learning or association. Thorndike states the problem clearly. Referring to one of Pavlov's cases of experimental extinction,[1] he says: "Ordinary learned connections do not act in this way. If a child has learned to respond to seven times nine by saying 'sixty-three,' and we ask him every two minutes, 'How much is seven times nine?' he does not as a rule become more and more halting and inaccurate and, after a dozen repetitions, fail."[2]

Using Thorndike's case, we may phrase the matter in this way. The *primary* stimulus to the answer "63" is visual (the printed characters of the multiplication-table, as read by the child); the *secondary* stimulus which he sets up in his own organism during the learning process is, let us say, some pattern which functions via proprioceptive channels. After the material is learned, this proprioceptive pattern functions in lieu of the primary visual stimulus whenever the child is asked the product of 7 x 9. This combination (secondary proprioceptive stimulus—verbal response) seems to

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Communication No. 9 from the Animal Hearing Research, Department of Psychology. These investigations are maintained by aid of the Research Council of the American Otological Society. Special grants from the Josiah Macy Jr. Foundation, from the Rockefeller Foundation, and from the American Academy of Arts and Sciences are gratefully acknowledged. Part of the material of this paper was read before the Midwestern Psychological Association, May, 1932 (cf. *Psychol. Bull.*, 29, 1932, 569).

[1] I. P. Pavlov, *Conditioned Reflexes*, 1927, 49. In this case stimulation by the metronome was not followed by actual feeding, so that the C-R was not reënforced. In successive trials at 2-min. intervals, the secretion of saliva in drops was as follows: 10, 7, 8, 5, 7, 4, 3.

[2] E. L. Thorndike. *Human Learning*, 1931, 109-110

grow stronger with repetition or use, so as eventually to become quite independent of the original visual equation in the book. With no further reference to the printed table, the child continues to reply year by year, "7 x 9 = 63." We find little trace here of experimental extinction. How different the situation in conditioning! A dog is conditioned to salivate when visually stimulated by meat-powder at a short distance. Five times at 3-min. intervals, the powder is shown without primary reënforcement (actual feeding); the conditioned secretion concurrently drops from 1.0 to 0.0 cc.[3] Likewise one of our dogs, as reported below, is trained to respond to a tone by retracting the paw from a charged grid. As soon as the animal responds 100 per cent to the tone, all reënforcement ceases. Thereupon extinction proceeds so precipitously that when the second period is reached, the animal fails to react a single time in the 25 trials (0%).

In this paper we hope to analyze the problem experimentally and to find whether this apparent opposition really exists. A failure in response may imply either of two quite separable causes: (a) some failure in the pattern itself; or (b) some failure of the incentives which normally activate that pattern. In experimental extinction, the dog may cease responding not at all because the conditioned pattern has been disrupted but simply because we fail to supply the energy needed to arouse it. Even an intact mechanism will stop functioning if we turn off the power.

Experimental procedure. Three female shepherd dogs were used as *S*s. They were trained by the methods regularly employed in this Laboratory.[4] The animal is placed in a stock, the right forepaw resting on a grid which can be charged with electricity; the secondary stimulus is a 1000-cycle tone, continuing for 2 sec. and terminated by the shock-to-paw. The conditioned response, by which the dog signals that she hears the tone, consists in withdrawing the paw while the tone is on and thereby escaping the imminent shock. A buzzer, placed in parallel with the grid, sounds sharply for the exact duration of the electric charge. In each of the daily work-periods the dog received 25 of these sound-shock combinations. This continued until she satisfied our criterion of 25/25 C-R in one period (100%). At the next work-period extinction was begun and continued until the animal failed to reply with a single C-R in the 25 trials (0%). Extinction was effected in two ways: (a) conditioned stimulus alone, no shock and no buzzer; (b) conditioned stimulus followed as usual by buzzer, no shock. In dog A the C-R was extinguished 6 times, order ababab; in dog B, 4 times, order abba; in dog C, 4 times, order baab.

Quantitative results. The essential figures, which are shown with full graphic detail in Fig. 1, are as follows:

(1) Time required for establishing the C-R in test-periods (of 25 trials each):

[3] Pavlov, *op. cit.*, 58.

[4] E. Culler, G. Finch, E. Girden, and W. Brogden. Measurements of acuity by the conditioned-response technique, *J. Gen. Psychol.*, 12, 1935, 223-227.

Dog A 17 2, 5, 2, 10, 7, 5.
Dog B 16 3, 3, 4.
Dog C 19 3, 4, 2.

(2) Time required for extinction:

Dog	Without buzzer	With buzzer
A	2, 4, 3	12, 8, 26
B	4, 3	18, 10
C	5, 7	17, 13

It will be seen that reëstablishment is accomplished in about one-fourth to one-fifth of the time needed for the original training. The mean time for extinction *without* buzzer is 4.0 periods, *with* buzzer is 14.9 periods. The difference of 10.9 has a standard error of ca. 2.39, hence is significantly greater than zero.

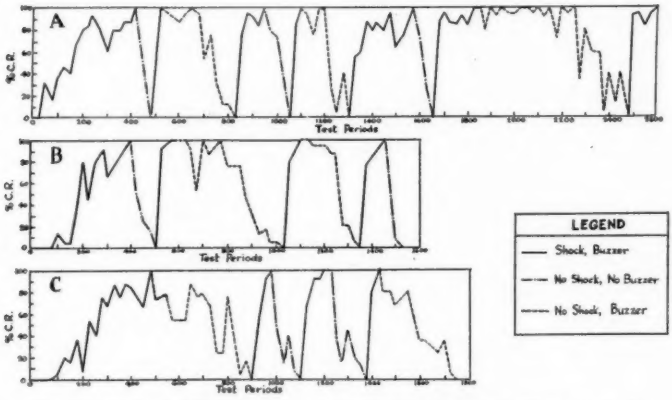

FIG. 1.—DATA ON EXPERIMENTAL EXTINCTION IN THREE DOGS, A, B, C

The abscissa shows the cumulative total of individual trials; the ordinate shows the percentage of conditioned responses in each test-period of 25 trials.

Interpretation. Inasmuch as presence or absence of the buzzer markedly affects the rate of experimental extinction, we must enquire how this effect is produced. Let us first be clear that the buzzer is *neither* a primary (unconditioned) *nor* yet a secondary (conditioned) stimulus to foot-withdrawal. The first of these propositions hardly requires proof; but let us examine the second. Repeated tests both of the present animals A, B, C, and also of numerous other dogs trained in the same way (shock always coterminous with buzzer), indicate that the buzzer alone has never become a secondary stimulus for the C-R. Though the dog has been reacting 100% for weeks to the tone, she will not, in even a single case, react to the buzzer. Upon observing the animal's behavior, we see why this is true: the buzzer serves rather to *terminate* than to elicit the C-R. If the buzzer be omitted in

a given trial, the animal continues to hold her foot flexed in air or else replaces it with obvious caution and hesitance; the paw being ofttimes applied lightly and hastily to the grid as though to try it out before actually depositing the foot. If, however, the buzzer be sounded as usual, the foot is quickly and firmly replaced and the dog visibly relaxes. Far from being a stimulus to foot-withdrawal, the buzzer becomes the cue to *foot-replacement*. The buzzer, when it stops sounding, makes known that the trial is over and that the grid again is safe. Just as the tone announces that the grid will soon be 'hot,' so the buzzer announces that it will soon be 'cold.'

By omitting the shock and continuing the buzzer, which has throughout the training-period been coterminous with it, we are in a way tricking or deceiving the animal. The buzzer has come to symbolize charge-on-the-grid; it continues now to do so, even though it has become a false symbol. Like any symbol, it assumes in some degree the characteristics of its object and may function, in lieu of the shock, as a substituted incentive. The curve of extinction may therefore assume any course between two extremes: (a) when the substitutive motivation is sufficiently potent and persistent, the C-R will remain on the same level indefinitely, and 'extinction' will follow the horizontal line in Fig. 2; (b) in case the substituted incentive wholly ceases, the C-R will fall at once to zero (vertical line).

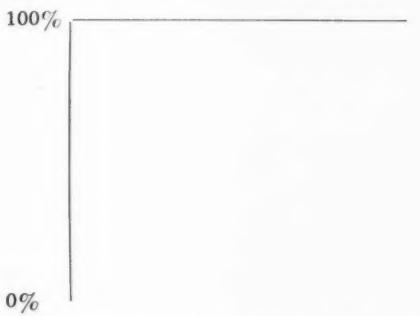

FIG. 2. POSSIBILITIES OF THE CURVE OF EXTINCTION

Somewhere within the bounds of this quadrant all extinction-curves must of necessity lie. No one of them can be called typical, save as certain conditions of motivation are typical. The C-R is by no means a mere stereotype which automatically falls at a certain rate when the primary stimulus is omitted; it can describe any path within the above quadrant, as determined by the amount of residual incentive after the unconditioned stimulus

is withdrawn. In an earlier study,[5] motivation was sustained by a thoracic shock after removal of the primary shock-to-paw; and there was every indication that a given C-R could be held steady at 100% for indefinite periods in these conditions. Here the buzzer substitutes for the foot-shock, but unlike the thoracic punishment loses its effectiveness at varying rates; and thereby serves even better to illustrate the functional correlation of experimental extinction with degree of substitutive incentive.

The primary stimulus (shock) plays a dual rôle in conditioning. (a) It determines the character or *pattern* of the response. Foot-withdrawal is clearly a function of the type of stimulus (electric charge) and also of its locus of application (right fore-paw). (b) It provides the *incentive* or drive needed to actuate this response-pattern. In the earlier study mentioned above, it was shown that motivation can be transferred to a new stimulus (thoracic) while the pattern is still being determined by the original stimulus (pedal). By thus separating those factors which provide the response-pattern from those which provide the drive, we get the set-up found in ordinary learning.

(1) Pattern of response
  (a) In conditioning—due to primary stimulus
  (b) In learning—due to primary stimulus
(2) Energy which activates response
  (a) In conditioning—due to primary stimulus
  (b) In learning—independent of primary stimulus; it usually proceeds from social rewards and penalties.

By merely omitting the primary S in conditioning, we automatically abolish all incentive for the C-R to continue; whereas in learned material, omission of the primary cue has nothing to do with incentives. In the case of conditioning, why should the dog continue the meaningless pantomime of hastily withdrawing his paw from a harmless grid? But in the case of learning the child does not get his incentives to remember 9 x 7 from the printed table where he reads it. These are provided by teacher and parents who praise him when he replies correctly, and by the classmates who laugh at him when he fails. If motivation is made *independent* of the primary stimulus, the C-R likewise continues indefinitely without abatement and higher orders can be readily added. In experimental extinction the animal does not stop reacting because he has suddenly 'forgotten' how to respond; after a period of rest the C-R is again restored. The whole picture reminds us of a reservoir being depleted and then refilled during the rest-interval;

[5] G. Finch and E. Culler. Higher order conditioning with constant motivation, this JOURNAL, 46, 1934, 596-602.

in other words, the available drive has become subliminal during extinction and must be renewed ere it will again set the mechanism in motion. It must be remembered that every individual leg-flexion is work done against impedance; as in engineering, every expenditure of power implies a load. The dog does not withdraw his paw 'for fun;' he is always overcoming resistance and therefore tends, unless specifically activated, to revert to quiescence. That is clear from the way extinction develops. If the shock be omitted, he may react as usual a few times; then as fatigue or inertia increase, he reacts slower than before. Nothing happens. Then he may delay still more, responding only after the shock-period is past. Again nothing happens. Even when he fails to react at all, nothing happens. Thereafter extinction becomes precipitate. (In dog A, for example, the first extinction ran as follows (25 trials): CCCCC - CCC - CC - - - - C - - - - - - - - - . In the next period of 25 trials, not one C occurred.)

We are well aware that the response herein designated by the usual abbreviation C-R is not a real conditioned *reflex* (*e.g.* salivation, eyelid, patellar). This very fact has made it useful for bridging the apparent gap between 'pure' C-R and learning, between extinction and forgetting; and thereby for revealing the factor which both distinguishes and relates them. That factor is *motivation*. This we suggest as the clue to their apparent discrepancies. Dashiell, in his recent survey,[6] has observed that whereas learning directly involves the *whole* organism, conditioning in its pure form refers to a particular *segment*. It is the implication of this difference which now concerns us. The energy which activates true conditioned reflexes can be derived only (or largely) from metabolic processes within their own restricted segment of the organism; hence their notorious instability and their need for repeated rehabilitation. Learning, however, can draw upon the vast resources of the whole organism. Fed by neural power imported from somatic and visceral receptors beyond its own immediate system, the typical learned pattern is quite independent of the primary stimulus which must always be kept at hand to resuscitate the failing C-R. Amount of *available energy* is the crucial factor. When learning is restricted to the energy generated within its own system, it exhibits the phenomena of true conditioning; when conditioning is adequately energized, it becomes identical with learning.[7]

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[6] J. F. Dashiell. A survey and synthesis of learning theories. *Psychol. Bull.*, 32, 1935, 261-275.

[7] Dunlap's intriguing suggestion for destroying undesired habits, speech-defects, tics, and the like by wearing them out seems to be effective for the same reason that experimental extinction is effective; it is a method of diverting the incentives which keep the habit-symptoms alive (Cf. Knight Dunlap, *Science*, 67, 1928, 360-62).

Conclusions

(1) True forgetting implies a disruption or impairment of the learned pattern; whereas experimental extinction implies that the pattern, though still intact, is not being adequately energized. Extinction is often (perhaps always) the first step in forgetting, because the pattern when quiescent is more easily blocked and disintegrated by competing and interfering processes. But though it be the first stage on the way to actual obliviscence, experimental extinction is not properly a case of forgetting at all. In this distinction lies the explanation for the apparent discrepancies between the two.

(2) It is further suggested that motivation is the general factor which both separates and unites the phenomena of learning and of conditioning. Learning, when deprived of general organic motivation, becomes conditioning; conditioning, when adequately energized, becomes learning.

EXPERIMENTAL EXTINCTION OF HIGHER-ORDER RESPONSES

By W. J. Brogden and Elmer Culler, University of Illinois

Finch and Culler showed, in a recent study,[1] that conditioned responses could be established up to five orders, and presumably to any desired order, by the use of a general energizing stimulus (thoracic shock). In this study we are concerned with the *relative strength* of the orders thus established, as measured by the rate of experimental extinction.

Three mongrel dogs, from 12 to 18 mo. old when training began, were used for the tests which continued through 20 mo. The stimuli of the several orders (first to fourth) were provided by a 1000-cycle tone, a 50-watt lamp, an electric bell, and an electric fan. The tone was of constant intensity, about 70 decibels above the animal's threshold; the light faced the animal about 5 ft. away; the bell was of the common telephone type; the fan played a stream of air directly into the dog's face, and hence was both visible and audible as well as perceptible via the skin-receptors.

First-order conditioning was thoroughly established by methods regularly employed in this Laboratory (shock to forepaw).[2] Before passing to a higher order, the stimulus to be used in that order was always tested for indifference; *e.g.* if the animal had already been trained to the tone, would it now respond without further conditioning to the fan or light as well? In one case a dog, trained to the first order, did react to the second-order stimulus prior to any experience with it. This animal was accordingly discarded. The dogs here reported were, prior to training, in every case known to be indifferent to the stimuli about to be used in the ensuing order. The thoracic shock was in like manner tested for indifference at the start of each new order; in not a single case did the animals react to it by foot-withdrawal. In higher-order conditioning, it should be noted, the crux lies always between first and second order (at the transfer from pedal to thoracic shock). Before the second order could be fully established therefore it was often necessary

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[1] G. Finch and E. Culler, Higher order conditioning with constant motivation, this Journal, 46, 1934, 596-602.

[2] Cf. *op. cit.*, 597.

to go back for one or more periods and to rehabilitate the first order. This reënforcement of the first order always occurred on separate days—never in the same period as the second-order trials. Once the gap from first to second order had been negotiated, no further delay was encountered. The third-order stimulus was applied along with the second, until third order alone evoked the response; then the fourth order with the third, until fourth order was established. When once thoracic motivation has been stabilized, the transition from one order to another is readily achieved.

In brief, the pedal shock was used only with first-order conditioning; in all higher orders, the thoracic shock alone was applied, the shock-to-paw not being employed in any case.

The test-period for both establishment and extinction comprised exactly 25 trials. In each order the C-R was built up to 92% (23/25) before extinction was begun. Thereupon the conditioned stimulus (first order) was applied without reënforcing shock for 25 trials daily until the criterion of extinction was achieved 8% or 2/25. The second, third and fourth orders were then successively extinguished in the same way.[3] After extinction of the four orders, they were again reconditioned by the original procedure. This subsequent retraining moved rapidly. One or two days (25 trials each) were quite enough to bring a given order back from 8% to 92%. After retraining, the four orders were once more extinguished; but now the second order was extinguished first, then the third, then the fourth, and lastly the first order. The following scheme shows the training and extinction schedule:

Order of training	Order of extinction			
Original training	1	2	3	4
First retraining	2	3	4	1
Second retraining	3	4	1	2
Third retraining	4	1	2	3

It is clear that each order was extinguished first, second, third, and fourth in each dog. Further to stabilize the process of extinction, a series of fixed time-intervals between stimuli was used as follows (sec.): 33—27—38—30—22—29—40—35—20—30—38—27—33—25—20—35—40—29—22—25—33—27—38—30. The elapsed time from the first to the last trial was therefore just 12 min. and 6 sec.

Results. Results on the higher-order conditioning were similar in all

[3] We used as our limits 92% and 8% simply to reduce the time involved. If one insists on a perfect score at both ends (100% and 0%) with all animals, the time may be increased by weeks; a given dog may undulate around 23/25 or 24/25 for days without giving a single 25/25. For present purposes it seemed pedantic to insist upon the ultimate scores. Kelley (*Quart. Amer. Statist. Ass'n.*, 17, 1921, 743) has well shown the value of this range (7% to 93%) for statistical purposes.

respects to those previously reported. A marked regression in the time needed for conditioning from the first to the fourth order was noted. Figures in days (25 trials each):

Animal	First order	Second order	Third order	Fourth order
A	28	18	5	3
B	25	19	6	3
C	46	22	4	3

As noted above, the retraining after each extinction took but a short time (1 or 2 days) and was about the same for all orders, first and higher. Summing all retraining for all dogs, we find that 1 day was required in 40 cases, 2 days in 7 cases, and 4 days in only 1 case.

In Table I is given the time of extinction in days for each order. By summing the scores for each extinction, a measure of the strength of the individual order is obtained. Since each order was extinguished in every position (first, second, third, fourth) the possible cumulative effects of earlier extinctions are roughly factored out.

Interpretation of results. As measured by our criterion, the first order is by far the most stable and persistent. The total scores for the three dogs are:

First	Second	Third	Fourth
232	62	64	62

Almost four times as many trials are required to extinguish it as to extinguish any of the three higher orders. Another way of putting the fact is, that upon comparing each first-order figure with all the higher-order figures for the same dog, we find the first order to be the greater in 133 out of 144 cases.

A second obvious fact is that the higher orders are almost identically equal, as indicated by the figures 62, 64, 62. The same is true for each of the three dogs separately considered. The maximal difference between the figures for the higher orders is in every case less than half its own standard error.

Animal	Maximal difference	S. E. maximal diff.
A	3.50—2.25 = 1.25	2.55
B	9.00—8.25 = 0.75	6.55
C	4.75—3.75 = 1.00	3.55

Statistically, the higher orders all belong to the same population; their stability as measured by the rate of extinction is indistinguishable. This is all the more notable in view of the large differences in time needed to establish the higher orders; from 18 to 22 days for the second, from 4 to 6 days for the third, and 3 days for the fourth. It indicates clearly that resistance to

TABLE I

ORDER OF CONDITIONING

The scores represent days needed for extinction. The letter in parenthesis after each score gives the position in which that order was extinguished. For example; under dog A, row 1, column 2, we read, 3 (*b*). This column is headed "light" which in the original conditioning was the second order stimulus, tone being the first. In the first of the series of extinctions, light was extinguished second (b) after the tone and prior to the bell and fan; and 3 days (25 trials each) were needed for extinction.

	Order 1	Order 2	Order 3	Order 4
Dog A	tone	light	bell	fan
Extinction 1	24 (a)	3 (b)	2 (c)	1 (d)
Extinction 2	5 (d)	3 (a)	4 (b)	1 (c)
Extinction 3	13 (c)	2 (d)	4 (a)	1 (b)
Extinction 4	8 (b)	1 (c)	1 (d)	11 (a)
Total	50	9	11	14
Mean	12.5	2.25	2.75	3.5
S.E.$_M$	4.20	0.48	0.75	2.50
Dog B	light	bell	fan	tone
Extinction 1	49 (a)	9 (b)	2 (c)	3 (d)
Extinction 2	29 (d)	22 (a)	7 (b)	22 (c)
Extinction 3	9 (c)	3 (d)	15 (a)	2 (b)
Extinction 4	23 (b)	2 (c)	10 (d)	6 (a)
Total	110	36	34	33
Mean	27.5	9.0	8.5	8.25
S.E.$_M$	8.30	4.60	2.72	4.66
Dog C	bell	fan	tone	light
Extinction 1	13 (a)	2 (b)	2 (c)	1 (d)
Extinction 2	40 (d)	6 (a)	5 (b)	1 (c)
Extinction 3	5 (c)	8 (d)	11 (a)	12 (b)
Extinction 4	14 (b)	1 (c)	1 (d)	1 (a)
Total	72	17	19	15
Mean	18.0	4.25	4.75	3.75
S.E.$_M$	7.60	1.65	2.25	2.75

extinction is not a function of the time required to constitute the conditioned pattern or mechanism; rather it is a matter of identical incentive in the three orders.

Pavlov states that the second order of the salivary response is definitely weaker than the first;[4] in defense reactions, the third was weaker than the second, which in turn was weaker than the first. It will be recalled that Pavlov denies the possibility of establishing the salivary C-R beyond the second and the defense C-R beyond the third order. Our results plainly confirm Pavlov's statement that the first order is by far the strongest, but among the higher orders themselves we found no difference. This we

[4] I. Pavlov, *Conditioned Reflexes*, 1928, 34.

ascribe to the use of a constant energizing stimulus. In our earlier study it was called "constant" merely because the shock remained physically constant in locus and intensity. Now we find it to be *psychologically* constant as shown by the persistence of the C-R based upon it.

We have still to explain just why the first order is so much more stable and persistent than any of the others. Let us look at the relative *excitation-value* of the two shocks, pedal and thoracic. To judge by the behavior of naïve animals, there is but little difference in the general disturbance evoked by the two. The characteristic picture after thoracic shock is something like this: wincing away from the electrodes, gasping, sag of the hind quarters, adduction of tail, followed by whining, biting, and efforts to escape. After pedal shock, the sidewise wince of the thorax and the momentary relaxation of the hind limbs are missing while vigorous flexion of the right foreleg (or of both forelegs alternately) occurs in their stead; but apart from the elements which are specific to the two shocks, the general picture (gasp, adduction of tail, whining, biting, defense- and escape-activity) is virtually the same. It is the concurrent opinion of our staff, all of whom have observed the effect of both shocks, that they are about equal in general effect. Both produce about the same vigor and variety of activity; both might, in other words, be called equally disturbing. It seems quite inadmissible therefore to ascribe the greater permanence of the first order to greater 'painfulness' of the foot-shock. Rather the problem is: Of two shocks which appear to be equally potent for inducing general somatic disturbance, why is one so much more effective in maintaining the specific response of foot-withdrawal? Two answers may be offered. (a) The pedal shock acts directly and therefore intensely into the *specific* leg-flexion mechanisms, whereas the specific movements released by thoracic shock have nothing to do with foot-withdrawal. There is no direct path of discharge from the left thorax to the flexion-musculature of the right foreleg. (b) It follows that the thoracic shock can act only indirectly, through the neural impulses initiated by proprio- and intero-ceptors as a consequence of general somatic activity common to both shocks. Further, the diversion of this neural energy to the conditioned flexion-response is itself a conditioning process, and requires time. In other words, the pathway which delivers the energizing neural impulses needed to keep the C-R alive must also be established by conditioning, instead of being ready-made as in the first-order foot-shock. The first order has therefore two distinguishable channels through which energy is fed into the C-R mechanism; the innate, specific path and the acquired, general path. The higher orders must depend on the second alone. It is then easy to see that the first order has a great

initial advantage in resisting extinction. It is, however, conceivable that this initial advantage could be overcome. If the channels through which the generalized (thoracic) drive operates were used long enough, they might eventually become functionally equal to the more simple and direct channels of the specific (pedal) incentive. It seems possible that the higher orders, sustained as they are by thoracic shocks acting through the more remote conditioned channels which carry the neural energy generated by widespread somatic activity, would in due time asymtotically approach equality with the first order. But in view of the many months covered by the present observations, this functional equality of the higher orders with the first would in any event require a long, long time.

In closing, an obvious analogy from the field of human behavior may be worthy of mention. It is notorious how much more effective, particularly with children, are the direct exciters (food, pain) than the more remote, complex incentives of social approbation and censure. These latter activators usually gain in potency as the individual grows older until they may even equal the former; which suggests again that the thoracic shock, if employed for long periods and with many kinds of stimuli, might eventually become just as effective as the primary foot-shock. In any event, the present evidence clearly indicates that two drives, though of equal value for releasing general activity, may be widely unequal in their value for a specific act. Discerning educators have noted that the satisfactions and penalties which are unique to a given act are more persistent and compelling than the universal incentives which society commonly employs. The thoracic shock, like spanking a child, might be used to maintain a whole repertory of conditioned behavior; but despite its convenience and universality, it is less effectual in ordinary conditions than drives which feed more directly into the conditioned mechanisms.

Conclusions

(1) The first order C-R is about four times as stable, when measured by its resistance to experimental extinction, as is any of the higher orders (second to fourth). This is true when the *general* excitation-value of the incentives employed (pedal shock and thoracic shock) is approximately equal.

(2) For the greater value of the pedal shock we suggest this explanation. Although it releases no more somatic activity than does the thoracic shock, far less of the neural energy generated by this somatic activity via intero- and proprio-ceptors is wasted in miscellaneous activity and far more discharges directly into the C-R pattern (foot-withdrawal).

(3) It follows that in all cases of motivation it is of utmost importance, not alone how much neural energy is generated by somatic activity throughout the body, but also how much of this neural flux is dissipated in irrelevant activity. The situation can well be stated in engineering terms. The efficiency of a given engine (incentive) is determined by the ratio, energy-expended-in-useful-work divided by total-energy-available.

(4) The higher orders (second, third, fourth) are definitely equal in resistance to experimental extinction. The energizing stimulus (thoracic shock) is thus proved to be *constant* not alone in its physical characteristics, but likewise in its psychological effects.

(5) The speed of extinction has nothing to do with speed of conditioning. Even though the time consumed in establishing a second-order C-R is six times as great as a fourth-order C-R, the time of extinction is precisely the same. Motivation being identical in the higher orders (thoracic shock), we conclude that extinction is a function of motivation alone; not a function of time nor of difficulty in establishing the C-R.

CARDIAC ACTIVITY DURING EMOTION

By N. Norton Springer, New York University

This study is concerned with the changes in the form of the electrocardiogram that result from sudden stimulation causing upset of an emotional nature.

Landis and Slight report a variety of changes in the form of the electrocardiogram following "surprise" stimulation.[1] They found that there was a tendency for stimulation occurring at certain points in the cardiac cycle to be effective in producing irregularities while stimuli occurring at other points in the cycle were ineffective.

Blatz,[2] in an earlier electrocardiographic study of cardiac activity during emotion, observed an "initial augmentation of the force of the heart beat (as measured by the amplitude of the ventricular contraction) which persists with but slight decrement for longer than six minutes."

In attempting to repeat these experiments, serious doubt was raised in the writer's mind about the nature of the changes of the wave-form reported by Landis and Slight. The experiment to be reported is divided into two parts. Part I is essentially a repetition of Landis and Slight's work. Part II is a double-galvanometer study to determine to what extent the distortion appearing in the heart curves is due to extraneous muscular activity.

Part I

Problem. Our problem in this part of our study was to ascertain the changes in the electrocardiogram, both as to form and time relations, that result from sudden stimulation causing upset of an emotional nature.

Subjects. Fifteen men, all college students in normal health, were used as the *Ss.*

Apparatus and procedure. A Hindle string-galvanometer was used for obtaining the electrocardiograms. The conventional electrocardiographic standardization and Lead II were used with all the *Ss.* Camera speeds of 25 and 100 mm. per sec. were used.

The usual technique of compensating for the so-called skin current was found inadequate because of a great shift of the galvanometer string following stimulation. In its place a 20 mf. condenser was connected in series with the string of the galvanometer, thus effectively blocking the skin current.

The galvanometer outfit and experimenter were in a room adjoining that of *S.* The stimuli were presented automatically from the galvanometer room. One of the stimulating devices selected was a tipping chair, following the example of Blatz.[3] This chair was so arranged as to rotate about the horizontal axis of the seat, thus

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[1] C. Landis and D. Slight, Studies of emotional reactions: VI. Cardiac responses, *J. Gen. Psychol.*, 2, 1929, 413-420.

[2] W. E. Blatz, The cardiac, respiratory and electrical phenomena involved in the emotion of fear, *J. Exper. Psychol.*, 8, 1925, 131.

[3] Blatz, *op. cit.*, 110.

giving the effect of removal of support. An auto horn of the klaxon type mounted under the seat of the tilting chair was used as a noise stimulus.

The experimental chamber in which *S* was placed could be completely darkened. The room, however, was not soundproof, and the faint hum of the camera and the motors of the time-marker could be heard. These motors were therefore turned on before *S* was brought to the laboratory and kept running continuously throughout the entire experimental period.

It was thought best not to let *S* know that the experiment was concerned with cardiac activity. He was told merely that the experiment was concerned with relaxation, and that the rate and degree of his relaxation would be measured. After the electrodes were bound to his limbs, he was instructed "to relax completely and to try to fall asleep." He was told further that the experiment would take half an hour, and was cautioned not to move under any circumstances as any sudden movement would damage the apparatus. The room was then darkened, and *E* left, closing the door after him.

After *S* had been connected to the galvanometer circuit and all adjustments made, *E* waited for a period of at least 10 min. When *S* was relaxed and the heart-rate was within normal limits, as shown by observation of the galvanometer string, a normal heart record was taken for 10 sec.; the stimulus was then presented and another record taken for 10 sec. Only one of the two stimuli was used with an *S*. In those experiments in which the tilting chair was used, *E* would enter the experimental chamber after the stimulus-record had been obtained, and tell *S* that nothing else would happen, but that he would have to lie quietly for another 15 min. He would leave the room and 15 min. later a normal record would be taken with *S* in the reclining position. The emotional stimulus was usually presented but once to every *S*. In three instances, however, the auditory stimulus was repeated. When this was done, at least 10 min. would elapse between successive presentations. A series of control records were obtained from 4 *S*s while they were contracting their fingers and shrugging their shoulders.

At the end of an experiment *S* was asked to give a detailed account of his feelings. An emotional state was held to have been secured only when *S*'s report warranted it, *i.e.* when he said, "I was frightened," "startled," "scared," etc.

Results. The electrocardiograms were analyzed with regard to (1) rate of every separate cardiac cycle; (2) maximum increase in the heart rate following stimulation; (3) the point in the cardiac cycle at which stimulation took place; (4) irregularities in the wave-form subsequent to stimulation; (5) time elapsing between stimulation and any resulting variation in the electrocardiograms; and (6) time relationship between systole and diastole as measured by the P-T and T-P intervals.

The effects of the emotional stimulus on cardiac activity are summarized in Table I. There is a rapid increase in heart-rate in 16 of the 18 records. This increase in rate reaches its maximum by the third or fourth beat following the stimulus, then the rate decreases irregularly. At the end of 10 sec. the rate has decreased considerably and in most cases has returned to the basal level.

The most interesting results are the irregularities in the electrocardiographic form following the stimulus. Sixteen of the 18 records show changes in the electrocardiographic form. The irregularities arise after an average latent period of 0.12 sec. and affect one or two beats. In individual cases these irregularities are present continuously or intermittently for the remainder of the record. None of these are

true, clear-cut cardiac changes. They consist of a medley of large irregular waves apparently of non-cardiac origin. The irregularities are similar to those introduced into the control record by voluntary contraction of the finger and shoulder muscles.

In the interpretation of all electrocardiograms it is imperative to distinguish between cardiac variations and those arising from other sources. Lewis points out that "it is perfectly true that many electrocardiograms show traces of variations produced by contractions of the muscles of the body wall or limbs and at times it may be difficult to exclude this complication from the curves."[4]

The introspective reports of the *Ss* indicated that when startled by the stimulus, there was a tendency to move the arms and body, and to clench the fists. These

TABLE I

EFFECTS OF THE EMOTIONAL STIMULUS ON CARDIAC ACTIVITY

(Single Galvanometer)

S	Stim.	Basal (Beats per min.)	Maxim. increase (Beats per min.)	Point stimulus occurred	Irregularities following stimulus	Latent period (secs.)	Effect of stimulus on P-T interval	Effect of stimulus on T-P interval
F	D*	56±4	46	T-P	Yes	0.04	None	Shortened, great variation
Z	D	66±4	34	P-R	Yes	0.04	None	" " "
K	D	74±5	26	P-R	Yes	0.04	None	" " "
W_1	N†	85±3	40	T-P	Yes	0.40	None	" " "
W_2	N	89±8	26	T	Yes	1.20	None	" " "
G	N	70±5	55	T	Yes	0.08	Slightly irregular	" " "
R	N	76±2	0	T-P	None	—	None	None
P	N	51±3	35	P-R	Yes	0.12	None	Shortened, great variation
Ro	N	75±2	32	T	Yes	0.28	None	" " "
M	N	76±3	18	T	Yes	0.10	None	" " "
K_7	N	59±1	29	T-P	None	—	None	" " "
Pe	N	70±4	0	S-T	None	—	None	None
Go_1	N	67±3	40	T-P	Yes	0.10	None	Shortened, great variation
Go_2	N	65±2	38	T-P	Yes	0.15	None	" " "
Ma	N	46±2	29	T-P	Yes	0.16	None	" " "
S_1	N	76±2	35	P	Yes	0.18	None	" " "
S_2	N	76±2	27	T-P	Yes	0.14	None	" " "
D	N	66±2	20	T-P	Yes	0.20	None	" " "

* Drop of the chair. † Noise of auto horn.

movements, the control records indicated, could produce the observed changes in the electrocardiogram.

The remainder of the cardiac cycles which do not show these irregularities do not show any changes in the systolic-phase. The individual components of this cycle are perfectly normal in all respects. The P-T interval is constant for the normal and stimulus-records in all cases except one. In the one exception it is slightly shorter following stimulation.

The diastolic-phase is markedly shortened by the stimulus. As the rate decreases

[4] T. Lewis, Auricular fibrillation and its relationship to clinical irregularities of the heart, *Heart*, 1, 1909, 331.

irregularly, the T-P interval shows concomitant variations. In two of the records the heart-rate is not increased and the T-P interval remains constant.

The two records which do not show irregularities in the form of the electrocardiogram do not show any variations in the duration of the P-T phase. The T-P interval is markedly decreased in one of them with the accompanying great increase in heart-rate, while it remains constant in the other records.

PART II

The results obtained in Part I seemed to imply that the changes in the form of the electrocardiogram following emotional stimulation were caused by muscular activity other than that arising from the heart. Our control records had shown that muscular activity in the arms and shoulders caused a distortion of the electrocardiogram that was very similar to the records obtained during "surprise" stimulation. We decided, therefore, to record simultaneously action currents from the arms and from the heart.

Problem. Our problem in this part of the study was to discover whether there was any activity present in the arm muscles following an emotional stimulation. If it were present, we wish further to know whether it caused a change in the form of the electrocardiogram.

Subjects. Seven young men, all college students in normal health, were used as the *S*s. The records from two of them were discarded because the normal electrocardiograms revealed cardiac abnormalities (*i.e.* ectopic rhythm and axial deviation).

Apparatus and procedure. A second string-galvanometer was used for the recording of the action currents from the arms. This galvanometer was so placed that the shadow of its string was recorded on the same film that was recording the electrocardiogram. Non-polarizable lead plate electrodes were bound to *S*'s forearm just below the elbow-joint. These electrodes were so arranged that any muscular activity in the flexores digitorum muscles of either the left or right arm would be recorded. The electrocardiograms were recorded with the apparatus described in Part I. The condenser methods of compensating for skin-current was used with both galvanometers. A camera speed of 50 mm. per sec. was used.

The instructions given *S* were the same as those in Part I. After *S* had relaxed and the heart-rate was within normal limits, he was told to contract either his left or right hand. These simultaneous records from the heart and from the arms were used as controls of the effect of extraneous muscular activity on the electrocardiogram. When *S* was relaxed a normal heart-record was taken for 10 secs.; then the stimulus was presented and another record was taken for 10 secs.

The auto horn was the stimulus used with all the *S*s. This procedure was repeated twice with every *S* so that 6 records (2 controls, 2 normal and 2 following stimulation) were obtained from every one of them. An interval of at least 10 min. elapsed between the successive presentations of the stimulus. In addition to the noise stimulus, records were obtained from 2 *S*s with the tilting chair.

Results. The electrocardiograms were analyzed for the same factors as those in Part I. The electromyograms were examined for the presence of muscular activity in the arms.

The effects of the emotional stimulus on the electrocardiograms and electromyograms are summarized in Tables II and III. There is a rapid increase in heart-rate in 10 of the 12 records. This increase in rate reaches its maximum by the 3rd or 4th

beat following the stimulus, then the rate decreases irregularly. By the end of 10 sec. the rate returns in most cases to the basal level.

Five of the records show marked irregularities in the form of the electrocardiogram following the stimulus. The electrocardiograms are distorted and many extra-waves are present. During the same interval the electromyograms show the presence

TABLE II

EFFECTS OF THE EMOTIONAL STIMULUS ON CARDIAC ACTIVITY

(Double Galvanometer)

S	Stim.	Basal (Beats per min.)	Maxim. increase (Beats per min.)	Point stimulus occurred	Irregularities following stimulus	Latent period (secs.)	Effect of stimulus on		
							P-T interval	T-P interval	
Sh_1	N	66±1	46	T	Yes	0.16	None	Shortened, great variation	
Sh_2	N	66±2	38	P-R	Yes	0.10	None	"	" "
Sh_3	D	75±2	45	T	Yes	0.04	None	"	" "
B_1	N	66±4	9	S-T	None	—	None	"Slight"	
B_2	N	68±3	0	T-P	None	—	None	Shortened, great variation	
N_1	N	66±5	0	T-P	None	—	None	None	
N_2	N	71	15	T-P	None	—	None	Shortened, great variation	
Sy_1	N	76±1	18	S-T	None	—	None	"	" "
Sy_2	N	66±3	20	R	Yes	0.20	None	"	" "
We_1	N	88±3	23	P-R	None	—	None	"	" "
We_2	N	88±4	14	R	None	—	None	"	" "
We_3	D	88±5	49	S-T	Yes	0.02	None	"	" "

TABLE III

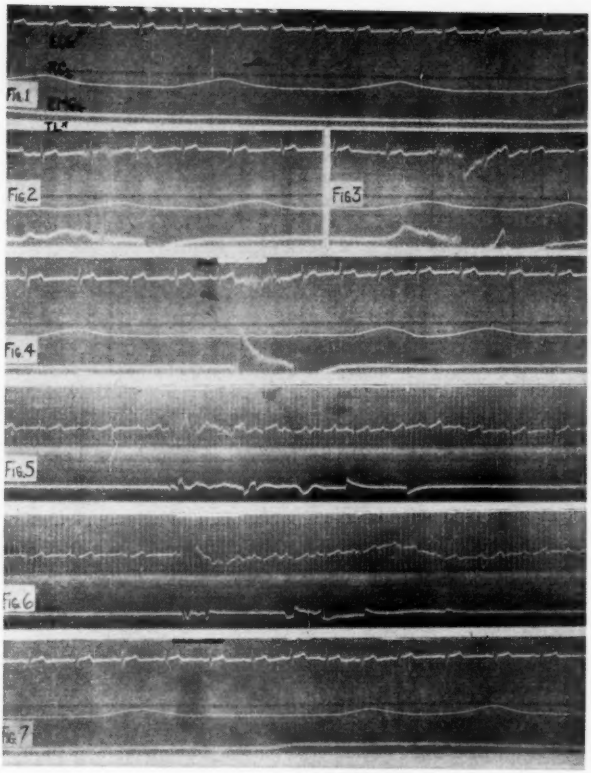
SUMMARY OF THE EXPERIMENTS IN WHICH SIMULTANEOUS RECORDS WERE OBTAINED OF THE ELECTROCARDIOGRAM AND ACTION CURRENTS FROM THE ARMS FOLLOWING EMOTIONAL STIMULATION

(Double Galvanometer)

S	Stimulus	Muscular activity in arms	Presence of distortion and extra waves in the electrocardiogram
Sh_1	Noise	Yes	Yes
Sh_2	Noise	Yes	Yes
Sh_3	Drop	Yes	Yes
B_1	Noise	No	No
B_2	Noise	No	No
N_1	Noise	No	No
N_2	Noise	No	No
Sy_1	Noise	No	No
Sy_2	Noise	Yes	Yes
We_1	Noise	No	No
We_2	Noise	No	No
We_3	Drop	Yes	Yes

of muscular activity in the arms. When the muscular activity in the arms subsides, the form of the electrocardiogram is normal and does not show any distortion. The changes in the form of the electrocardiograms and electromyograms are similar to the control records obtained when the subject was told to contract his hand.

Figures 1 to 7 show typical records obtained with the double-galvanometer



FIGS. 1–7. GRAPHIC RECORDS OF THE EXPERIMENT

FIG. 1. NORMAL RECORD. Each deflection of the electrocardiogram (ECG) P, QRS and T wave, is of normal direction, amplitude, duration and contour. The heart-rate (about 76 beats per min.) is almost constant. The respiratory curve (RC) is regular. The electromyogram (EMG) is from the right arm and does not show the presence of any muscular activity. The time lines (TL) are 0.02 sec. The film speed is 50 mm. per sec.

FIGS. 2 AND 3. CONTROL RECORDS. When *S* contracts his right hand the electromyogram shows a medley of small irregular waves. During the same interval the electrocardiogram shows similar distortion.

FIGS. 4–7. AUDITORY STIMULUS RECORDS. Following auditory stimulation, the electrocardiograms are distorted in Figs. 4, 5, and 6. During the same interval the electromyograms show the presence of muscular activity in the arms. The changes in the electrocardiograms and electromyograms are similar to the control records of Figs. 2 and 3. (The shift of the base line of the electromyogram in Figs. 5 and 6 is due to great changes in the skin current.) Fig. 7 does not show any changes in the form of the electrocardiogram following auditory stimulation. Outside of an increase in heart-rate, the electrocardiogram is perfectly normal. The electromyogram does not show the presence of any muscular activity in the arms.

technique. Figure 1 presents a normal record taken when the subject was completely relaxed. Figures 2 and 3 are control records which were obtained to determine the effect of extraneous muscular activity on the electrocardiogram. The subject was told to contract his hand. The electrocardiograms become distorted when the electromyograms show the presence of muscular activity in the arms. When the subject relaxes and no muscular activity is present in the arms, the electrocardiograms are perfectly normal and free from distortion.

Figures 4 to 7 present auditory stimulus records. Figure 4 shows that during the fore-period, each deflection of the electrocardiogram is of normal direction, amplitude, duration and contour. The electromyogram is a straight line and does not show the presence of any muscular activity in the right arm. Stimulation takes place during the R wave. After a latent period of 0.20 sec. a medley of irregular waves distort the electrocardiogram for about 2 beats. During this same interval the electromyogram shows the presence of muscular activity in the right arm. The changes in the form of the electrocardiogram and electromyogram are similar to the changes in control Figures 2 and 3. The distortion of the electrocardiogram disappears when the muscular activity in the arm stops. For the remainder of the record, outside of irregularities in the heart-rate, the electrocardiogram is perfectly normal and the electromyogram shows the absence of any muscular activity in the arms.

Figures 5 and 6 are similar to Figure 4. Following auditory stimulation there are marked simultaneous changes in the electrocardiograms and electromyograms. These changes are similar to those found in control Figures 2 and 3. The shift of the baseline of the electromyograms is due to great changes in the skin current.

Figure 7 is typical of the 7 records which do not show any changes in the form of the electrocardiogram or electromyogram following auditory stimulation. Outside of an increase in heart-rate the electrocardiogram is free from distortion and extra-waves. The electromyogram shows the absence of muscular activity in the arms.

In all 12 records the systolic-phase is constant in duration for the normal and stimulus records. The diastolic-phase is markedly shortened in all but 3 records following emotional stimulation.

Discussion

While these records confirm Landis and Slight's observation of changes in the form of the electrocardiogram during emotional stimulation, they indicate that such changes may be artifacts introduced by extra-cardiac activity. The experiments show that muscular activity of the arms and shoulders distorts the form of the electrocardiogram, and they indicate the presence of such muscle currents following sudden stimulation. When simultaneous electrocardiograms and electromyograms from the arm muscles were obtained, the heart records show distortion only when there is concomitant muscle movement. Landis and Slight do not mention other muscular activity. Unfortunately, they do not reproduce any of their electrocardiograms, and we cannot make any comparisons. We are inclined to believe that extraneous muscular activity is present in their records and has been misinterpreted for heart changes.

Landis and Slight report that these divergencies from the normal cardiac activity are more closely related to the temporal correlation between the time of stimulation and the point in the cardiac cycle at which the stimulation takes place than at any other factor. Our records, however, include stimulation in every part of the cardiac

cycle, and indicate that such cardiac changes appear to be conditioned more by the presence of muscular activity in the arms than by any other factor.

Besides these extra-cardiac changes, the cardiac cycles do not show any changes except the shortening of the diastole which accompanies the increase in heart-rate. The records which show the absence of muscular activity in the arms reveal only this shortening of the diastole and no changes whatever in the form of the electrocardiogram. Blatz reports an increased amplitude of the R-wave following a sudden backward fall. We cannot confirm these findings.

Summary and conclusions. The result of an electrocardiographic study on the effects of sudden emotional stimulation indicate that with the exception of great increases in heart-rate, the cardiac records that are free from extraneous muscular activity do not show any wave-form changes.

By means of a double galvanometer technique, simultaneous action-current records were obtained from the heart and from the muscles of the arm. The implication of these records is that the wave-form irregularities are not true cardiac changes, but are due to extraneous muscular activity.

The results indicate that the changes in the form of the electrocardiograms reported by Landis and Slight may be extra-cardiac and not fundamental in origin.

APPARATUS

A DIRECT-COUPLED AMPLIFIER FOR THE STUDY OF ELECTRODERMAL PHENOMENA

By PAUL E. GRIFFITH and M. A. WENGER, University of Iowa

The inadequacy of the comparatively slow-moving coil galvanometer in studying certain electrodermal phenomena has recently been emphasized by Davis.[1] He employed a direct-coupled amplifier based upon a circuit originally suggested by Arnold in 1915,[2] to which he added a vacuum tube in order to maintain a known and relatively constant current in the subject's circuit.[3] A Duddell oscillographic unit was used as the recording instrument. For work where photographic records were not essential, a common milliammeter was substituted.[4] Davis and Porter report that, in simultaneous readings with a milliammeter and a cathode-ray oscillograph, little difference was found. The milliammeter, then, when used with adequate amplification, represents a considerable improvement over the moving-coil galvanometer for most psychological and physiological work. Davis' apparatus has the additional merits of enabling the resistance of the subject to be read from the control dials without supplementary operations and of giving a linear output curve. It probably represents the most adequate of the inexpensive instruments which have been used in work with the so-called "phenomenon of Féré."

If it is also desired, however, to study the changes in apparent bodily potential following stimulation ("phenomenon of Tarchanoff") or the occurrence of those potential changes as measured by the tachogram of Godefroy,[5] alterations in Davis' apparatus are necessary. The present paper describes a modification of and additions to his circuit whereby all three phenomena may be studied by means of the same instrument. Fig. 1 shows the circuit of the amplifier.

It will be seen that three separate circuits are available. The first, for the study of resistance, represents only a modification of Davis' apparatus. A pentode has been used in the subject's circuit in order to increase the ratio of the resistance of the source of the subject's current to that of the subject. Thus the current through the subject is rendered even more stable than in Davis' circuit. For example, a drop of 3,000 ohms from a level of 20,000 ohms when a current of 50 microamperes is

[1] R. C. Davis, Factors affecting the galvanic reflex, *Arch. Psychol.*, 18, 1930, (no. 115), 1-64.

[2] H. D. Arnold, U. S. Patent 1,129,943, 1915. Cf. H. J. Van Der Bijl, *The Thermionic Vacuum Tube and Its Applications*, 1920, xix, 391 (patent listed in footnote, p. 253).

[3] Cf. R. C. Davis, A vacuum tube for stabilizing the current during measurements of the galvanic reflex, this JOURNAL, 41, 1929, 474-475.

[4] R. C. Davis and J. M. Porter, Jr., A measuring device for the galvanic reflex, *J. Gen. Psychol.*, 5, 1931, 115-120.

[5] J. C. L. Godefroy, The psycho-electro-tacho-gram and exophthalmic-goiter, *Psychiat.-Neurol. Bladen*, 26, 1922, 131-173.

used, results in a current change of only 0.13 microamperes or a change of 0.26%. The relative constancy of this current has also been increased by the inclusion of a fixed resistance of 50,000 ohms, R_9, as an automatic biasing resistor.

The use of a 6C6 tube connected as a pentode tube in the first stage of amplification serves to give the amplifier greater sensitivity.[6] This tube connected as a triode is well suited for use in the last stage of amplification because of its high mutual

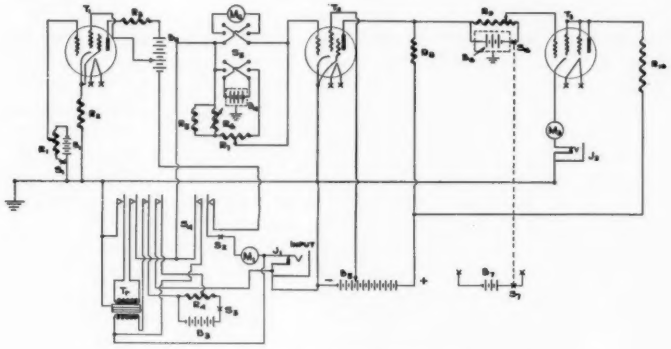


FIG. 1. SHOWING THE CIRCUIT OF THE AMPLIFIER

T_1 = current stabilizing tube for Subjects circuit, type 6C6
T_2 T_3 = amplifier tubes, type 6C6
M_1 = 0–200 microammeter, Weston 301
M_2 = 0–10 voltmeter, Weston 301, 1,000 ohms per volt
M_3 = 0–5 milliammeter, Weston 301
R_1 = 5,000 ohm potentiometer
R_2 = 50,000 ohm resistance
R_3 = 0.5 megohm resistance
R_4 = 25,000 ohm potentiometer
R_5 = 9,000 ohm resistance
R_6 = 2,000 ohm wire-wound rheostat
R_7 = 400 ohm wire-wound potentiometer
R_8 = 1.0 megohm resistance
R_9 = 100,000 ohm potentiometer
R_{10} = 50,000 ohm resistance
B_1 = 4.5 volt battery
B_2 = 180 volt battery
B_3 = 22.5 volt battery
B_4 = 9 volt battery
B_5 = 180 volt battery
B_6 = 90 volt battery
B_7 = 6 volt battery
S_1 = S.P.S.T. switch on R_1
S_2 = S.P.S.T. switch
S_3 = S.P.S.T. switch on R_4
S_4 = 4 P.D.T. switch
S_5 = 4 P.D.T. switch
S_6 = D.P.S.T. switch
S_7 = D.P.S.T. switch
J_1 = input jack
J_2 = closed circuit, extension jack for photographic recording unit
T_r = W. E. 208-B input transformer

conductance. Furthermore, the heater-cathode type of tube is desirable, for when such tubes are employed, not only may all of the heaters be operated from the same battery but the cathodes may be connected in any way desired. Thus the automatic biasing resistor mentioned above can be employed without using a separate battery to heat the filament. Also, the final meter, M_3, can be connected at ground potential.

[6] Should the sensitivity be too great for any given use, it may be reduced by varying the screen-grid voltage on tube T_2.

By such an arrangement not only is danger of damaging the meter through short-circuiting eliminated, but it also becomes unnecessary to shield the extension leads to a further photographic recording unit if one is used.[7]

In calibration and technique of operation this circuit of the amplifier does not differ from that reported by Davis and Porter. It is essential, however, that it and all of its leads that are at high electrostatic potential above ground be thoroughly shielded. External shielding is required, therefore, for batteries B_4 and B_6 and their leads and for the lead to the subject, which is not at ground potential. It is also essential, due to its greater sensitivity, that some very fine control of the grid voltage of tube T_2 be utilized. At present a 400-ohm wire-wound potentiometer is used. A still more precise control might be desirable. The sensitivity of the amplifier is of the order of 0.25 ma. change per mv. input change. The range of operation is 0-16 mv., with the range of 0-14 mv. maintaining a linear characteristic.

A modification of the above circuit provides for the measurement of changes in apparent bodily potential,[8] or the measurement of any change in potential of the order cited above. This as achieved by eliminating the current to *S*. The result is a millivoltmeter. It must be realized, however, that when a vacuum tube is used a small current is always flowing in the control-grid circuit. In the case of the 6C6 type of tube this current is of the order of 2×10^{-8} amperes. With this constant current flowing through a resistance of 20,000 ohms, a drop of 3000 ohms would result in a potential change of 0.06 mv., which would appear on the recording meter as 0.015 ma. change. Since a change of 0.05 ma. is approximately the lowest which can be accurately read, it is evident that if the external resistance in the circuit is low the amplifier functions, for all practical purposes, as a true millivoltmeter.

A further alteration in the millivoltmeter circuit makes possible the measurement of electrodermal activity after the method first described by Godefroy.[9] He used a one-to-one transformer in series with the subject and a battery in the primary circuit, and a Moll galvanometer with a period of 1½ to 2 sec. connected across the secondary. The present circuit also uses a one-to-one high impedance transformer,[10] the secondary of which is connected in series with the grid-biasing battery of the first amplifier tube and ground. The primary coil is put in series with a 22½ v.

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[7] It has already been stated that Davis used a Duddell oscillograph. Ruckmick and Grubbs have used a milliammeter and the shadow technique of recording. (See Gardner Murphy, *General Psychology,* 1933, 103). A unit has been constructed by the second writer for use with the present amplifier which consists of a highly damped 10-0-10 milliammeter, Weston No. 301. The glass face is removed and the needle bent at right angles to the face of the meter at a point as close to the pivot attachment as is practicable. An oscillograph mirror, 1 x 2 mm. in size, is then cemented to the tip of the needle, which is bent further until it rotates over the point of pivot, thus making no lateral excursions. The damping of the meter allows little fling, and its large range renders the actual rotation of the mirror small since only 5 ma. of the complete range are needed. By varying the distance of the unit from the camera the recorded responses may be reduced or amplified.

[8] According to Broxon and Muenzinger, changes in bodily potential as a result of external stimulation range from 0 to 17 mv., with a mean of 5 mv. as measured with a quadrant electrometer and nickel plated copper electrodes, 2.5 cm. in diam., applied to one palm. See James W. Broxon and Karl F. Muenzinger, Changes in skin potentials during the psychogalvanic reflex, *J. Gen. Psychol.,* 5, 1931, 94-98.

[9] *Op. cit.*

[10] The impedance of the primary at 60 cycles was 21,000 ohms.

battery and the subject. The current flowing through the subject may be controlled by adjusting potentiometer R_6.

Davis has criticized the Godefroy circuit because of the distortion involved in the use of ordinary transformer coupling with wave forms having a slow rate of change. Its use can still be justified, however, when frequency of response is the main object of study and particularly when the responses are being photographically recorded. The method has an advantage in that no adjustment is required to return the recording beam or shadow to a 'zero' base-line after a large drift in apparent resistance has carried it off the film, a phenomenon which quite frequently occurs with newborn human infants and with adults at high resistance levels. This feature, combined with its sensitivity of reaction to slight changes in contact resistance or potential, makes the transformer-coupled circuit a serviceable addition to any psychological laboratory. For example, it can be employed for continuous recording of very slight movements of the body against the attached electrodes without the necessity of making subsequent adjustments. Godefroy has already reported that the pressure of the pulse against the subject electrodes is sufficient to deflect the recording instrument. Observations made with the present amplifier corroborate his findings.

The switching arrangement by which any one of the circuits described above may be used consists of switches S_2, S_4, and S_5. With S_4 thrown so as to connect the subject to the constant current circuit and with S_2 closed, the equipment is connected to measure resistance. S_5 is thrown to give a positive potential whenever the IR drop across the subject's resistance is greater than the biasing voltage (E_g) used to obtain a linear output curve during calibration. When it is less, S_5 is thrown to provide a negative potential. For example, with the present amplifier, in which E_g is one volt negative, when a current of 50 microamperes is used, resistance below 20,000 ohms would require a negative potential. All measurements above 20,000 ohms would demand a positive potential.

Resistance measurements are then obtained by adjusting R_6 and R_7 until meter M_2 is at the reference point determined at the time of calibration and reading from meter M_3 the voltage (E_m) used to neutralize the subject's potential. Then $R = (E_g - E_m)/I$ when the IR drop in the input circuit is less than E_g, or $R = (E_m + E_g)/I$ when the IR drop is greater than E_g. It is obvious that since the current may be kept constant throughout an experiment, only E_m need be recorded. Measurements of resistance made in this manner are accurate to within the accuracy of the meters. If more exact measurements are required, the amplifier may be used as a comparison instrument and a calibrated resistance-box switched in for the measurements.

When the amplifier is to be used as a millivoltmeter, S_2 is opened and S_5 is thrown to the position which biases the grid negatively. Deflections of M_3 may then be converted to millivolts by reference to the calibration curve.

When the transformer-coupled circuit is desired, the millivoltmeter arrangement requires only a slight change. S_4 is thrown to its opposite position and S_3 is closed.

AN APPARATUS FOR MEASURING THE ELECTRODERMAL RESPONSE

By D. Ulrich Greenwald, University of Iowa

The following characteristics are desirable in devices used for measuring changes in the electrodermal response, or the so-called "psychogalvanic reflex:" (1) measurement in standard physical units; (2) continuous measurement; (3) a permanent record of all variations; (4) constant visibility of the resistance level and the variations in response; (5) ruggedness; (6) sensitivity adequate to research work; (7) portability; (8) simplicity of operation; (9) a minimum of electrode polarization; (10) simple, preferably dry, electrodes; (11) as constant a current as possible through the subject (*S*); and (12) reasonable cost. An apparatus with these characteristics would possess most of the advantages of the improvements made to date. Such a device has been recently constructed and is described here.

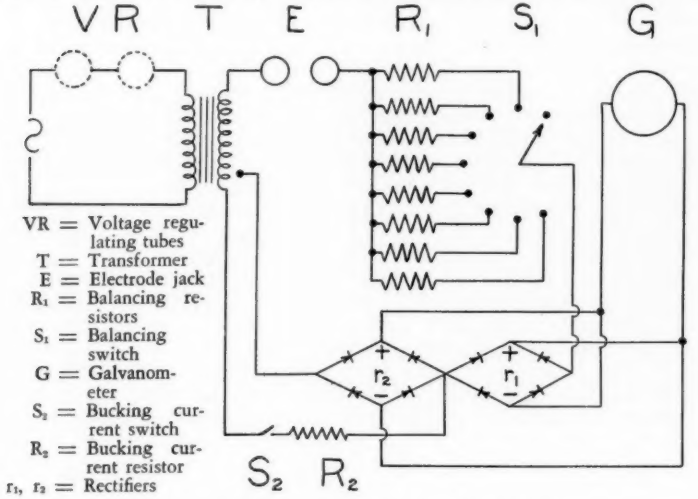

Fig. 1. Circuit Diagram of the Apparatus

The electrical circuit is fairly simple. Essentially 60-cycle current from a commercial lighting supply is reduced in potential by a transformer to a usable value. The alternating current passes through *S* and another much larger resistance. This resistance has a definite fixed value to match a rectifier and galvanometer in series, so that the drop in potential across the resistance determines the current through the meter. The alternating current passes through a copper-oxide rectifier into the direct current galvanometer. Another supply of direct current, opposite in potential to the measuring current, comes from a special low voltage tap on the transformer,

is rectified by a second copper-oxide unit, and keeps the galvanometer within its scale limits. A resistance (R_2) determines the amount of this current used.

A diagram of the measuring circuit is shown in Fig. 1. The primary leads of the transformer (T) were supplied with a 110-volt commercial current. Where the commercial voltage is unstable, voltage regulating tubes (VR) in series may be inserted. The secondary of T delivered a potential of 15 volts. R_1 contained a number of fixed-value, precision-wound resistors which were thrown into the meter circuit by means of an eight-point, shorting type, rotary switch (S_1). This same switch also closed the 'bucking' circuit through switch S_2. The values of R_1 were selected so that for an *S*'s resistance of 15,000 ohms and each multiple of that resistance, up to eight, exactly 135,000 ohms were placed in the secondary circuit of the

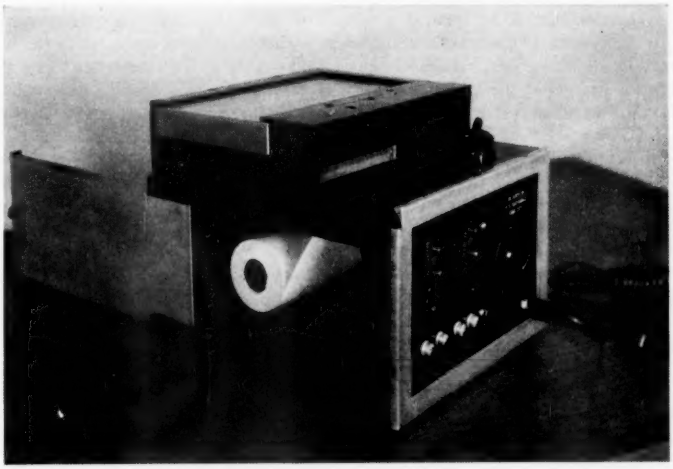

FIG. 2. THE ASSEMBLED APPARATUS
Panel door opened to show photographic recording apparatus.

transformer. Thus the resistance of *S* plus the fixed resistance always approximated 135,000 ohms and resulted in a maximum current variation through *S* of about 8%.

The rectifiers were of the type usually found in A.C. milliammeters. They could rectify a current as great as 1 ma. continuously and had a negligible resistance. The galvanometer had a sensitivity of 0.13 microamperes per mm. of scale deflection and with the proper 'bucking' current would give a full scale deflection for a drop in *S*'s resistance of 15,000 ohms. It was therefore possible to detect changes in dermal response as slight as 500 ohms at any resistance level up to 135,000 ohms. More sensitive and expensive galvanometers are available which would permit a still more constant current through *S*.

In order to return the galvanometer to its zero position, a 'bucking' current with a potential of 5 v. was obtained from another tap on the transformer and the current

rectified through another copper-oxide unit. This potential ran through a resistance (R_3) which determined the amount of the opposition current.

The calibration was made by placing a precision resistance box at E after the apparatus had been permanently installed in its case. A calibration curve was drawn for each of the 8 positions of switch S_1, the scale readings of the galvanometer becoming abscissae and the corresponding ohmages at E the ordinates. These curves were very nearly linear.

To obtain continuous readings of all galvanometer deflections as a permanent record a camera using 4 in. bromide (Eastman No. 1) sensitized paper was built as an integral part of the device. The whole apparatus was housed in a rectangular box 11 x 11 x 9 in.

The left side of the box (Fig. 2) shows the camera while the right side contains the electrical circuit. In front is a translucent scale upon which a small concave mirror mounted on the recording instrument reflects a beam of light. Inside the box and directly below this scale this beam is focussed through a narrow slit upon the sensitized recording paper. The supply spool carries a 200 ft. roll of the paper which travels over a roller directly in front of the recording slit and down to a 'take-up' spool which is driven by a small constant-speed motor within the box at the right of the camera. This motor drives the 'take-up' spool at 2½ r.p.m. with a torque of 2½ ft.-lbs. Continuous records 20 min. in length are possible and require about 15 ft. of film. A sliding aluminum panel-door makes the camera light-proof and yet accessible. A similar door on the top of the box allows access to the electrical circuit and motor.

The switch S_1 and other controls are located on a bakelite panel on the front of the box. They include besides S_1, a switch for the motor, one for the light-beam, a set of binding posts for a stimulus-key, another set for the timing instrument, and a jack for the electrode leads. If the circuit between the 'stimulus' binding posts is closed, a flash of light records a spot on one edge of the photographic record. A momentary closed circuit between the 'timer' binding posts produces a line across the photographic paper. Thus a mercury contact metronome connected to these posts would give a time-line on the record of any desired interval. The power-lead enters the box through a plug on the top. The electrodes used with this apparatus were of the type described by Ruckmick.[1]

The operation of the box is as follows. After the camera has been loaded it is set on a table at some convenient reading height and the power lead is plugged into a commercial outlet. *S* is connected to the electrodes and the leads are plugged into the jack. The light switch is closed. Switch S_1 is turned until *S*'s resistance can be read on the scale. If a continuous record of response is to be made, the motor and timer are started and *E* needs to attend to the apparatus only to make sure that the line of deflection stays on the scale. When it reaches either end, it is brought nearer to the center by changing the position of S_1. These changes cause a sudden sweep on the record which is easily recognized and checked against a record kept of the settings of S_1. To note particular points in the record *E* presses a stimulus-key which records a spot on the photographic paper. At the end of an experiment S_1 is set at zero, *S* is disconnected, the motor and light circuits are broken, and the record is developed.

[1] C. A. Ruckmick, A new electrode for the Hathaway galvanic reflex apparatus, this JOURNAL, 42, 1930, 106-107.

With two minor exceptions this apparatus possesses all the advantages listed in the first paragraph. It does not produce as constant a current through *S* as do some D.C. devices. An adequate degree of constancy is obtained, however, by having a large resistance is *S*'s circuit at all times so that the maximal variation possible is 8%. Secondly, the device does not read directly in ohmage units. It is necessary to consult a calibration curve to convert the mm. scale-readings into ohms. For general use, however, these various curves are nearly enough linear so that scale readings may be compared directly.

A PORTABLE CHRONOSCOPE

By Earl Allgaier, Iowa State College

Most standard chronoscopes such as the Dunlap, Seashore, Bergström, Hipp, Hathaway, Ewald, Klopsteg and others either require alternating or interrupted current, or are so delicate mechanically that jar or movement invalidates their calibration. It is not easy to use a tuning fork or interrupter in a moving vehicle. For the reasons given the chronoscope described below was devised for use when conditions are not favorable to a more exact instrument. To meet the needs of the experiments, a dual release system makes possible the recording of three successive intervals of time with a probable error of approximately 0.01 sec. in each measurement.

The chief advantages of this instrument are that it is portable and always ready for use without assembling or making adjustments. Mounted in a durable case 14 x 13 x 7 in., it is completely self-contained, requiring no outside source of energy. The chronoscope may be used in a moving vehicle or under similar conditions of motion where many chronoscopes could not be used.

The mechanical parts are simple, the most complicated being the phonograph motor and an electromagnet which operates a release mechanism. Since the instrument is simple, strongly built, and has only one adjustment, trouble is reduced to a minimum. A very small amount of current, obtained from 3 dry cells contained in the case, is needed to operate the electro-magnet. If 6-volt d.c. is available the batteries may be eliminated.

Readings may be taken directly from the dials (see Fig. 1) with a probable error of 0.01 sec. This accuracy is sufficient for the use of the chronoscope in actual situations. As the readings are in terms of 0.01 sec. there is no need of a calibration scale. Instead of calibration, it is only necessary to adjust the speed of the motor to one revolution per sec. This may be done with reasonable accuracy with a stop watch, but is easily accomplished with extreme accuracy by stroboscopic means.

Where greater accuracy is not required, this instrument may be used for the measurement of reaction to light or sound or any stimulus desired. It is convenient for class room demonstration, since it is always ready for use without adjustment.

When necessary the chronoscope will measure 3 successive time-intervals. In our experiments the first interval is the time required to move between two predetermined points on a road. From this measurement the speed of the vehicle may be readily determined. The second interval is the time required for the driver to react to a

light or sound signal. The third interval measured is that required for the vehicle to come to a stop after the brakes have been applied. Since the speed of the vehicle has been previously determined, it is a simple matter to calculate the rate of deceleration. From the deceleration the efficiency of the brakes can be readily determined. This latter measurement would be useful for the checking of brakes in a hurry under actual road conditions. By use with a decelerometer the relative influences of the

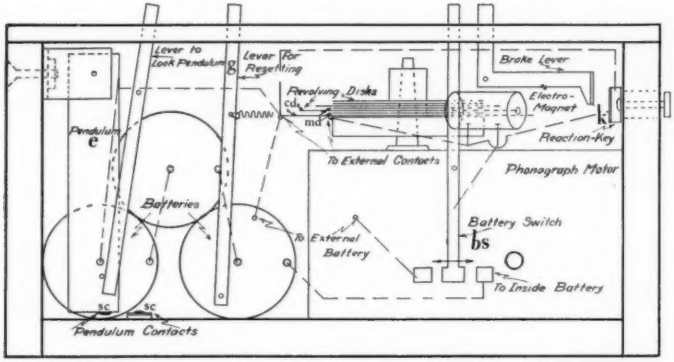

FIG. 1. DIAGRAM SHOWING SIDE VIEW OF THE PORTABLE CHRONOSCOPE

driver and the mechanical aspects of the vehicle may be resolved from the latent period in stopping time.

The essential feature of the present chronoscope[1] is a phonograph motor which turns, by friction, 4 celluloid disks. The friction is obtained by metal disks (md) which rotate at all times with the motor and which are alternated with the celluloid disks (cd), see Fig. 2. The speed of the motor is adjusted so that one revolution is made each second. The disks are graduated into 100 units, each unit representing 0.01 sec. The 'release mechanism,' (r), shown in Fig. 2, holds the disks from turning while the motor is running at constant speed. Whenever the reaction-circuit is closed the magnet allows the 'release mechanism' to move one notch so that one of the celluloid disks is released and allowed to rotate with the phonograph motor. When the circuit is closed a second time, the second disk is released. The time elapsing between the release of the two disks is measured by the distance the first disk has passed over the second disk. This reading may be taken directly from the graduated scale on the second disk. For example, in Fig. 2 the first disk was released 0.21 sec. before the second. The second disk has been released for 0.12 sec. while the third and fourth disks have not yet been released.

The pendulum (e, Fig. 2), located in the lower left hand corner of the instrument case, may be used to close the circuit where it is desired to measure the time at which a vehicle begins to decelerate or the time at which it comes to a stop.

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[1] Developed under the direction of Dr. A. R. Lauer as part of the activities of the National Research Council's Committee on the Psychology of the Highway.

For simple reation-time to sound, the motor is started with all of the disks held by the 'release mechanism,' (r, Fig. 2). The stimulus may be given by pressing the 'reaction-key' (k, Fig. 2), which in turn releases the inside disk. *S* responds by pressing a telegraph key connected with the 'external contacts.' This releases the second disk so that the time elapsed, or reaction-time, may be read from the inside scale. The motor is stopped for taking the readings by means of the 'brake lever.'

When used in a motor vehicle all of the operations are automatic. Two blocks are set up on the roadway at a measured distance apart. The second block contains

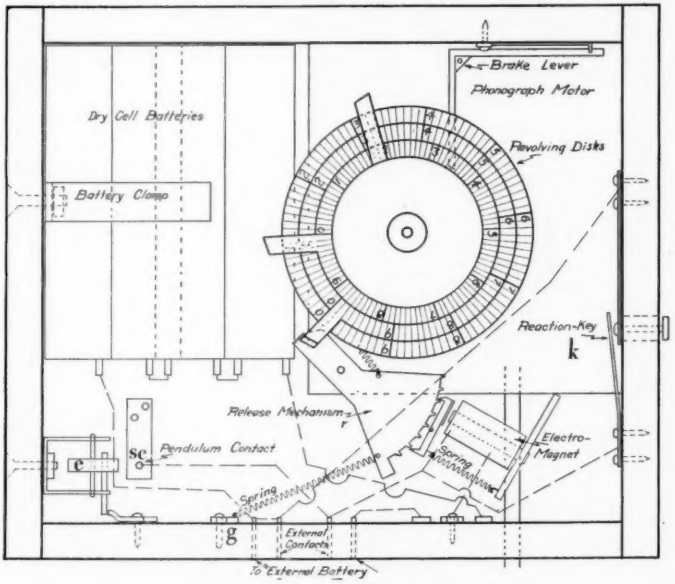

FIG. 2. DIAGRAM SHOWING TOP VIEW OF THE CHRONOSCOPE

a switch which turns on a light or rings a bell as the vehicle passes over. The chronoscope is placed on the floor of the car and the phonograph motor started before the test begins. Two wires run from the 'external contacts' to the front bumper of the car, and are there attached to a switch which is closed when the vehicle passes over each of the two blocks. *S* drives at a uniform speed until the circuit operating the light is closed. As the vehicle passes over the first block, contact is made and the first disk is released. In passing over the second block the second disk is released, and simultaneously the circuit which operates the light is closed. As soon as the driver reacts to the light and is able to get his foot on the brake, the vehicle begins to decelerate. This deceleration swings the pendulum forward, making contact with the 'pendulum contact' (sc, Fig. 2) and thus releasing the third disk. When the vehicle comes to a complete stop the pendulum again

swings back of the contact and releases the fourth disk. By stopping the phonograph motor the 3 intervals of time may be taken directly and from them may be calculated the speed of the vehicle, time of reaction of the driver, and efficiency of the brakes of the vehicle.

If a 6-volt current is available it may be connected to the points labeled 'to external battery' (Fig. 1), eliminating the necessity of dry cells. The 'lever to lock pendulum' (e, Fig. 1), is used to prevent the pendulum from swinging when not in use. The 'lever for resetting' (g, Fig. 1) is used at the end of each test to reset the 'release mechanism,' so as to hold the disks from revolving. The 'battery switch' (bs) enables the operator to use either current from dry cells within the case or from outside 6-volt direct current at will.

A METASCOPE AND DIPLOSCOPE

By DAVID P. BODER, Lewis Institute, Chicago

In the discussion of Stratton's classical experiment on vision without inversion of the retinal image,[1] and of the results of similar researches recently reported by Ewert,[2] and Peterson,[3] the student is impressed with the fact that perceptual and motor adjustments can be made to an "upside down" world. Brief consideration of the problem of visual transposition indicates that the discrepancy between the perceptual and the real object is not of equal consequence along all diameters of the visual field. If, while driving an automobile, I notice an object to my right, a turn to the left will be sufficient to avoid a collision, whether this object is perceived in its proper vertical position or "upside down." If the object is actually located at the left but is seen, due to an illusion-producing device to the right, then the turn to the left will not adequately meet the situation.[4] It appears, therefore, that the vertical transposition is less grave than a horizontal one. In order to demonstrate this fact, and also for the purposes of research, the apparatus described here—an image-transposer, a combined metascope and diploscope[5]—was devised.[6]

The apparatus consists of two principal parts: an optician's trial frame, and a pair of prism-holders each of which contains two 90° prisms. The prism-holders are

[1] G. M. Stratton, Vision without inversion of the retinal image, *Psychol. Rev.*, 4, 1897, 341-463.

[2] P. H. Ewert, A study of the effect of inverted retinal stimulation upon spatially coördinated behavior, *Genet. Psychol. Monog.*, 7, 1930, 177-363.

[3] Reported at the Nashville meeting of the Southern Society for Philosophy and Psychology (cf. J. Peterson, this JOURNAL, 47, 1935, 513).

[4] Similar inadequacies are demonstrated with the pseudophone (cf. P. T. Young, Auditory localization with acoustical transposition of the ears, *J. Exper. Psychol.*, 11, 1928, 400-429).

[5] These terms, metascope and diploscope, mean respectively, "image-transposer," and "image-doubler." The Greek derivations have been adopted to bring this apparatus into the family of optical devices, *e.g.* telescope, microscope, pseudoscope, stereoscope.

[6] The writer is indebted to Mr. Joseph H. Stoneking for his skillful coöperation in the construction of the instrument.

made of a light brass and have a circular base fitting into the trial frame (Fig. 1). For each eye two prisms are used, such as found in field glasses. They are put together at their hypothenusal planes separated by a thin piece of white but opaque paper. The prism-holders are then set into the trial frame, and by proper adjustment a fairly large horopter at a distance of 4–5 ft. can be obtained.

In front or behind the plane of fixation, the objects appear double but with such clearness that *S* is rarely aware of the duplication unless he looks at people, when the duplication is immediately noticed as a rather uncanny sight. Otherwise duplica-

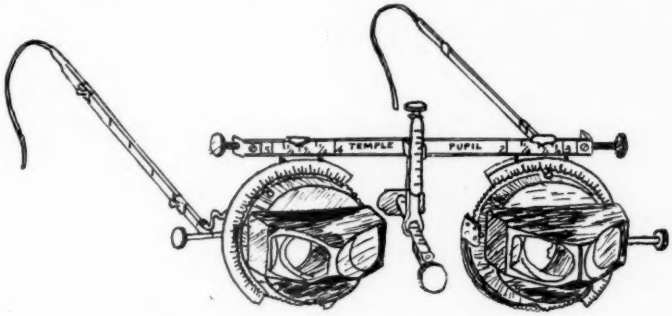

FIG. 1. A COMBINED META- AND DIPLOSCOPE

tion of objects, especially in a laboratory environment, does not represent necessarily anything disturbing. On the other hand there is a large monocular field for each eye which supplements the horopter, increasing the amount of objects seen singly.

I. THE METASCOPE

Position 1. If the prism-holders are placed horizontally, an apparently normal picture is obtained. It is only when *S* begins to enumerate the perceived objects in a horizontal order, viz. from right to left, that the reversed perception in the horizontal diameter becomes known to *E*. *S* himself is not aware of the abnormality until he begins to point at objects or attempts to follow with the hand a horizontally moving object, for example, *E's* hand holding a pencil. In drawing a star the errors and difficulties appear exactly contrary to the mirror drawing situation. In the latter the antero-posterior direction is reversed, the right-left remaining normal, while through the metascope (with lens-holder in the horizontal diameter) the antero-posterior direction is normal and the right-left orientation is reversed.

Position 2. If both lens-holders are placed vertically, an up-down reversal is obtained, but the right-left position remains normal. Past pointing takes place, but the pointing is correct as to laterality; if the object is sufficiently extended in the vertical direction, it will be touched correctly although not exactly at the intended place. As a whole it appears that a much quicker adjustment is made with vertical displacement than horizontal.

At least two genetic factors appear to contribute to such more rapid adjustment. (a) In many cases it is irrelevant whether the object is grasped at any definite point. If I perceive my fork prongs toward myself instead of away from me, but the fork

as such is at the proper side of my plate, I can still find it more easily than when I see it at the left side of the plate while in reality it is at the right. (b) The upside-down reversal is in most cases recognized immediately as in perfect discord with our perceptual set; a human being, furniture, etc., seen upside-down with the ground upward and the sky or ceiling downward immediately creates an attitude of caution and slowing down of the motor responses which soon assume the character of trial and error learning. With the right-left reversal *S* rarely notices that such a reversal has taken place at all, unless he begins a motor act or observes printed matter. It seems that it was this right-left reversal and not the up-down one which was responsible for most of the learning time in Stratton's and Ewert's experiments.

The present construction of the apparatus does not permit one readily to obtain an horopter with the up-down reversal at a distance nearer than 6-8 ft. For nearer objects, the shifting of both lens-holders nasalward or temporalward will render a composite field of the two monocular fields without *S* noticing that such is the case. In pointing towards an object he is not aware that it is seen monocularly, still less with which eye he sees it. However, by slightly converging the lens-holders and adjusting the frame properly, a narrow horopter may be obtained, even nearer than the indicated distance.

Position 3. If the lens-holders are placed diagonally but parallel to each other, the field of vision appears to be turned 90°. Standing individuals appear to be lying in the air in a horizontal position without support. In toto, starting with the horizontal position of the lens-holders and then turning them in the socket 180°, the student may be shown the definite change of localization of the object with change in the retinal position of its image. The conditions for obtaining an horopter at near or far distance are more favorable than in Position 2.

The preliminary experiments performed with the apparatus seem to indicate that as a whole an up-down reversal is more easily overcome than a right-left reversal, and that there is some consistency in error for each subject when learning to draw a figure, viz. a diamond, with these glasses on. The drawing experiments were performed with monocular vision since with the present apparatus binocular single vision could not be readily obtained at the usual writing distance. It is also worthy to note that the discrepancy between muscular and visual cues in drawing had upon a few *S*s such violent effect that they appeared near a condition akin to sea-sickness.

II. The Diploscope

The metascope can also be used as a diploscope. The beginner in psychology has considerable difficulty in obtaining a double image without instrumentation. As said before, our apparatus gives a single binocular image only when the two lens-holders are on a straight (horizontal) line or nearly parallel in any other direction and then only for the plane for which they are adjusted. If, however, the lens-holders are brought out of alignment the objects are seen double and in different positions. Moreover, if a staircase or cube illusion is observed, not only is the picture seen double, but the illusion is frequently, although not always, disparate, *i.e.* one staircase will appear as seen from above while the other will appear as seen from below.

Work with the diploscope indicates that the shift in the form of perception in the stair-case and similar illusions may be determined by some peripheral changes in addition to generally assumed central factors such as attention.

THE PANTOGRAPH AS AN INSTRUMENT FOR STUDYING KINAESTHESIS

By J. F. WALKER, University of Arizona

In a study of sensori-motor abilities recently undertaken,[1] the writer used the pantograph for the purpose of measuring kinaesthetic perceptivity and was convinced that it is a valuable instrument in such studies.

The pantograph was firmly attached to the lower left hand corner of an ordinary drawing board, 18 x 36 in., to which had been added, at the upper left hand corner, a trapezoidal piece, 6 x 16 in. for the accommodation of the traveling point of the

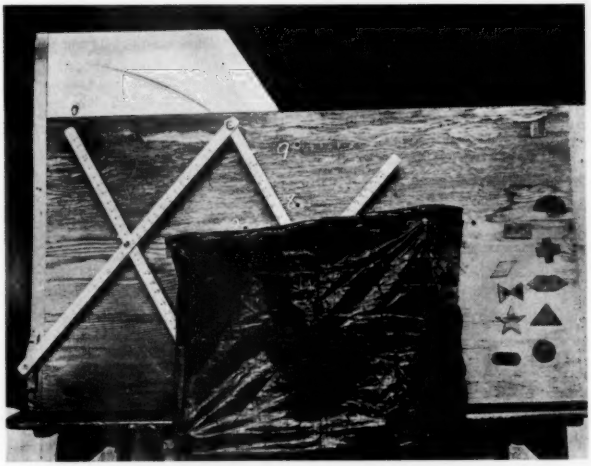

FIG. 1. PANTOGRAPH MOUNTED FOR USE

pantograph (see Fig. 1). Two holes were drilled in the drawing board in such positions as to support, by inserted dowel pins, a screen which was constructed of opaque cloth stretched over a light framework. This screen was so placed that *E* could see the form he was tracing, but *S* could see neither the form nor his own hand, which held the pencil attached to the pantograph. When *E* moved the stylus around the form being presented the *S* allowed his hand to move freely with the pantograph pencil.

The pantograph was set so that the object traced by *O*'s hand was about 50% larger than the one traced by *E*.

[1] J. F. Walker, unpublished dissertation in the University of California Library, Berkeley, California.

Two tests, A and B, each having two forms, were prepared and administered to 75 *S*s (12-yr.-old children) who were in attendance in the public schools of Martinez, Calif.

In Test A, *S* indicated his perceptual ability by attempting to identify each of the 10 objects which were of the same shape as the ones which he had kinaesthetically observed. In Test B, *S* was required to make a line drawing of the object which he perceived his hand to outline. The objects originally chosen for Form 1 of the pantograph object test were those of the Goddard form board,[2] but it was found that the *S*s could not detect any kinaesthetic difference between the square and the rectangle, both of which were included in the Goddard forms, and a form similar to a longitudinal cross section of an hour glass was substituted for the square. Form 2 was made up of 10 objects differing in details from those in Form 1. The pattern to be traced by the *E* was made by cutting the outline of the forms of the objects from heavy drawing paper and pasting this upon a glass so that the stylus ran in a glass-bottomed path bounded on both sides by heavy paper. *E* could guide the stylus around this path and at the same time observe the behavior of *S*. The pencil was also allowed to operate upon a glass plate so as to eliminate friction.

In Test A, objects of the same size as those traced by the pantograph were made in wood and placed before *S*, who was directed, after each trial, to pick up and to show *E* the one he thought he had traced. After each such trial the object was returned to its original position and the next trial given. The objects were always in *S*'s view, and he was told that each object might be drawn several times. As *S* indicated the object that he thought he had outlined he was scored. The reliability of this test with 12-yr.-old children was $0.52 \pm .06$, while the objectivity, found by having two *E*s administer the test to 25 children in the Fairmont School, was $0.45 \pm .12$.

In Test B, in which *S* was required to make a line drawing of the figures he had perceived, a grading system was devised for measuring the quality of the response. The reliability of the test was found to be $0.83 \pm .02$ and the objectivity $0.59 \pm .10$ when given by the same *E*s to the same groups as were used with Test A. The difference in reliability and in objectivity of the two tests was largely due to the fact that in the pantograph object test the score depended upon the "all or none" principle, while, in the pantograph drawing test, partial success was credited in accordance with definite printed instructions.

In experimenting with an *S* it was found possible to discover to what extent kinaesthetic sensitivity had developed unilaterally. The *S* even showed some ability in kinaesthetic perceptivity by means of the toes of the right foot.

The instrument could be used for studying cross education as well as ordinary kinaesthetic sensitivity and seems to be well worth experimenting with in sensorimotor studies.

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[2] H. H. Goddard, The form board as a measure of intellectual development in children, *Training School Bull.*, 9, 1912, 49-52.

APPARATUS NOTES

Properties of Electrodes Used in Recording Action Potentials from the Intact Organism

Three principal arrangements of electrodes have been used in recording action potentials from skeletal muscles in intact organisms. In one, the so-called "monophasic" arrangement, one electrode is placed near the source of potential, either on the skin or beneath it, and the other, relatively large, at a considerable distance from it. In the second, perhaps commonest arrangement, two electrodes are placed near the source, but separated from each other by some distance of the order of a centimeter. In the last few years concentric needle electrodes, first used by Adrian and Bronk, have been favored by a number of investigators.[1]

The electrical properties of these various dispositions seem never to have been carefully investigated. It is important to know their effects, since three main features of the resulting record are determined by electrode position. First, of course, the size of potential difference obtained will vary enormously with electrode position. In the second place, the position of electrodes may determine whether the record will show principally phenomena arising in one locality, or record almost equally phenomena of a wide area. Finally, it is well known that the arrangement of electrodes plays a large part in determining the form of the waves recorded.

Action potentials in the body arise in an effective bath of electrolyte, and it is the implications of this fact that need to be considered. Basing their work on the distribution of potentials in volume conductors of infinite extent Wilson, Macleod, and Barker have given the equation for the potential of a single point as a function of its distance from a "source and sink."[2] (On the basis of the work of Craib[3] and Wilson[4] the conception of action potentials as consisting of dipoles seems to be correct.) Neglecting the third dimension, which makes very little difference in the relationship, the equation is:

$$V = c' \left[\left(1/\left[(x-a)^2 + b^2\right]^{1/2}\right) - \left(1/\left[(x+a)^2 + b^2\right]^{1/2}\right) \right] \quad \ldots\ldots\ldots [1]$$

in which a is half the distance between the anode and kathode of the source, x is the horizontal distance from the midpoint of the dipole (on the line connecting the poles) and b is the vertical distance from that point. The term c' is a constant representing the size of the potential at the source, and conductivity of the bath. From this the equation of the potential difference between any two points in the field may readily be written. Letting l represent the distance between electrodes on the x axis and m the distance between them on the y axis, we have:

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[1] E. D. Adrian and D. W. Bronk, The discharge of impulses in motor nerve fibers, *J. Physiol.*, 67, 1928, 119-151.

[2] F. N. Wilson, A. G. Macleod, and P. S. Barker, The distribution of the currents of action and of injury displayed by heart muscle and other excitable tissues, *Univ. Mich. Stud., Sci. Ser.*, 10, 1933, 1-57.

[3] W. H. Craib, Study of electrical field surrounding skeletal muscle, *J. Physiol.*, 66, 1928, 49-73; The electro-cardiogram, *Med. Res. Council, Special Report*, 1930, (no. 147).

[4] F. N. Wilson, The distribution of the potential differences produced by the heart beat within the body and on its surface, *Amer. Heart J.*, 5, 1930, 599 f.

$$V = c' [(1/[(x - a)^2 + b^2]^{1/2}) - (1/[(x + a)^2 + b^2]^{1/2}) - (1/[(x + l - a)^2 + (b + m)^2]^{1/2}) + (1/[(x + l + a)^2 + (b + m)^2]^{1/2})] \quad [2]$$

It may be observed that when l and m become large with respect to the other variables the last two terms become negligible and Equation [2] reduces to Equation [1]. Equation [1] then represents the relation for the "monophasic" lead, and Equation [2] for two leads, with either small or moderate distance between. The curves, in Fig. 1 accompanying, represent these equations graphically. In Curves I and II, c' is taken equal to 1, $a = 1$, $b = 1$, $l = 1$, and $m = 0$. In Curve III, l is taken equal to 0.1, other constants being the same. These curves have been verified by introducing potential from a pair of dry cells into a pan of tap water on electrodes 1 cm. apart, and exploring the surrounding field with a vacuum tube voltmeter device.

To compare the curves first with regard to magnitude, it will be seen that with the constants chosen, the size of the peaks in Curves I and II is about the same. It would be possible by choosing l as some value greater than $2a$ (depending on the value of b) to make the main peak of Curve II greater than the peak of Curve I.

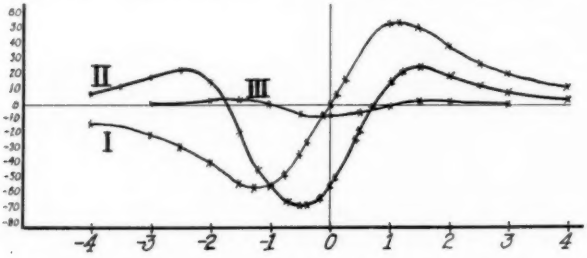

FIG. 1. GRAPHIC REPRESENTATION OF THE EQUATIONS

As a practical accomplishment in recording this would probably not be possible. Curve III shows that narrowing the gap between electrodes sharply decreases the magnitude of the peaks. While it is true that the smallness of voltages on leads may be overcome by increased amplification in the recording device, it is obviously undesirable to reduce obtained potentials unless corresponding advantages are thereby secured.

Sharpness of localization of phenomena is desirable in almost all action potential work. This means that ideally potentials originating near the electrode or electrodes should be large, and those at a distance relatively small. On the graphs this would mean that there was a sharp peak and relatively small tails. Actually for sharpness of peak there seems to be little to choose between Curves I and II. Curve III is definitely inferior to the others in sharpness of peak, as the negative phase is almost as wide as in Curve II and only one-tenth as far from zero at the minimum. This obtains when the distance between the electrodes is small with reference to $2a$, the distance between the potential peaks as they lie in the tissues, as appears to be the case with the concentric needle electrodes. The effect of increasing b, the perpendicular

distance from the line joining anode and cathode, is almost the same as that of increasing x. The potential becomes relatively smaller and less sharply localized. Regarding localization, therefore, the curves indicate first, that there is little to choose between the situations represented by Curves I and II; and secondly, that potentials relatively near the electrodes are not sharply differentiated from those even slightly farther away, and that those at greater distances are also not differentiated from each other.

Sharpness of localization with a single active electrode was verified by the following experiment with a human subject. The active electrode was placed on the right forearm. The inactive was a saline bath on the right foot. These were connected to an amplifier and oscillograph. Oscillating potentials from an audio oscillator, 100 c.p., were applied to the surface of the arm by electrodes respectively 1 and 2 in. away from the active electrodes. Potentials 10 times the size of those ordinarily recorded from the muscles were applied through skin punctures without any discernable effect on the record.

Wave-form of the recorded potential may also be discovered from an examination of the curves in Fig. 1 since most near-by excitations would be expected to travel along the line connecting the anode and cathode and pass under the recording electrode or electrodes. A single dipole under those circumstances would inscribe a curve like those of the graphs. Since Craib's work,[5] however, it seems that a single excitation is represented by a pair of dipoles, with the order of signs reversed. The records of such a phenomenon would therefore be the curves here shown plus an inverted duplicate, with more or less overlap. The "monophasic" type of lead would yield four similar phases; leads with two active electrodes would yield six phases, dissimilar in size, as Craib has shown. On the grounds of avoiding duplication of phenomena as much as possible, the single active lead would appear preferable.

The discussion has implied that the concentric needle leads may be treated as two active electrodes with a very small gap. The situation is even more complicated, as one almost surrounds the other, but the bearing of the argument would appear to be unchanged by that fact.

On all three considerations the single active electrode seems to be preferable. Question may be raised about the potentials arising near the supposedly inactive electrode. If that electrode is large such potentials would be expected to average out to zero. If the inactive electrode is a saline bath the lead may be taken from the edge of the bath, still further insuring an effective zero. The electrocardiogram is also something of a problem with this type of lead. In general this will appear between any two distant bodily points because of the relatively enormous voltage generated at the heart. The resultant wave may, of course, simply be neglected; or it may be avoided by attaching the inactive lead to a point on the body which is iso-electric as far as the heart is concerned with respect to the active electrode. Such a situation would be approximated with both leads on the same limb. Further reduction of the electrocardiogram may be obtained by dividing the inactive electrode, placing half of it at some distance away from the active in the direction of the heart, the other in the opposite direction. By varying the distance between the inactive electrodes it should then be possible to balance out the electrocardiogram entirely.

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[5] Craib, *op. cit.*

Parallel Recording of Vertical and Horizontal Oscillations by Means of an Erecting Prism

Many present laboratory techniques call for the simultaneous recording of horizontal and vertical oscillations of a beam of light. Such recordings are desired in eye-movement cameras or in devices for recording tremors of the hand or finger. The present techniques require two sensitive films traveling at right angles to each other or else a complicated system of mirrors.

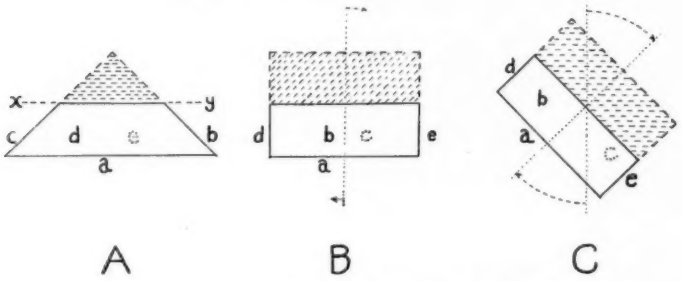

Fig. 1. Three Views of a Simple Erecting Prism

The simple *erecting* prism shown in Fig. 1 makes it possible to simplify many of the technical problems involved in recording these or related phenomena. Through its use one of the recording films may be dispensed with and the system of mirrors may be greatly simplified. In Fig. 1 the base is labeled as face *a*. Faces *b* and *c* form 45° angles with face *a*. Faces *d* and *e* form 90° angles with faces *a*, *b*, and *c*. The cross-hatched section of the prism as shown in the diagram may be removed to make a Dove prism as it is not used in the system. Such a removal will permit a more compact arrangement of the entire light-system.

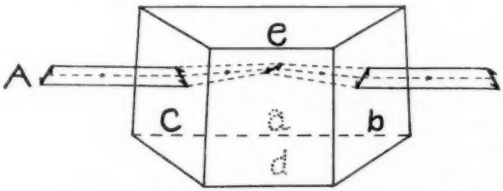

Fig. 2. The Path of a Beam of Light Through an Erecting Prism

When this prism is in the position shown in Fig. 1 A, any beam of light passing through the prism parallel with the faces, *a*, *d*, and *e*, will be inverted. If, however, the prism is rotated about the axis *xy* as indicated by the arrows, the image will be rotated twice as rapidly as the prism. For each degree of rotation of the prism, the image of the beam of light will be rotated two degrees. If the prism be rotated 45° to the position shown in Fig. 1C, the beam of light, passing through parallel to the

faces *a*, *d*, and *e*, and the axis of rotation *xy*, will be rotated 90°, or from the vertical to the horizontal plane. In a like manner, any beam of light passing through the prism in this direction will have its vertical oscillations rotated so that they will emerge from the prism in the horizontal plane.

Fig. 2 indicates the path of a beam of light through the prism when the prism has been placed in the 45° position. The beam of light **A**, which is oscillating in the vertical direction on the left side of the diagram, emerges on the right side of the prism oscillating in the horizontal direction. The beam of light entering the prism at face *c* is refracted to face *a*. At face *a* the light rays are reflected to face *b* where they are again refracted on emerging from the prism. The rotation of the prism varies the point of entry of the beam of light while oscillating, which in turn alters the point of reflection on face *a*. This accounts for the rotation of direction of oscillation of the beam of light.

University of Iowa ROBERT Y. WALKER

NOTES AND DISCUSSIONS

Influence of the Thyroid Gland on the Consciousness of Time

That man is able to estimate the passage of time fairly accurately is attested by every day experience as well as by results from carefully controlled experiment. Certain individuals, for example, are able to awaken at a predetermined hour,[1] to state the time of day on demand without reference to environmental changes,[2] and to gauge or reproduce time-intervals with great exactness.[3] An appreciation of duration is also strikingly shown in the accuracy with which post-hypnotic suggestions are timed.[4]

How these phenomena occur is as yet undetermined. Many theories, however, have been advanced in explanation of them. Some authorities posit 'duration' as a fundamental aspect, attribute, or dimension of experience,[5] and refer to it as the basis of the temporal judgments. Others hold to the view that the judgments are derived from the synthesis of sensory data, chiefly strain sensations,[6] and still others that they result either from an awareness of cyclic bodily changes, *e.g.* the rhythms of circulation and respiration,[7] or from the chemical changes of metabolism.[8]

Since the thyroid gland influences, as is well known, the coarser rhythm of circulation and the more diffuse and intricate rhythms of metabolism, the thought occurred to the writer that changes in thyroid activity would be paralleled by variations in the estimation of temporal intervals if the 'time-consciousness' were actually dependent upon those factors. The following experiments were, therefore, undertaken.

The *Ss*, 125 in number, were patients selected at random from the Thyroid and Medical Clinics of the Presbyterian Hospital in the City of New York. They were brought, one at a time, into a small, quiet, and artificially illuminated room; addressed in a friendly manner, "Good morning, will you sit down;" and then given two tests.

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[1] L. D. Boring and E. G. Boring, Temporal judgments after sleep, *Studies in Psychology: Titchener Commemorative Volume,* 1917, 255-279. E. N. Brush, Observations on the temporal judgment during sleep, this JOURNAL, 42, 1930, 408-411.

[2] G. S. Hall, The estimation of time, this JOURNAL, 7, 1895, 450 f.

[3] Herbert Woodrow, The reproduction of temporal intervals, *J. Exper. Psychol.,* 13, 1930, 473-499; Individual differences in the reproduction of temporal intervals, this JOURNAL, 45, 1933, 271-281; Mary Sturt, Experiments on the estimate of duration, *Brit. J. Psychol.,* 13, 1923, 382-388; *Psychology of Time,* 1925, 120 f.

[4] M. W. Richardson and J. M. Stalnaker, Time estimation in the hypnotic trance, *J. Gen. Psychol.,* 4, 1930, 362-366.

[5] E. B. Titchener, *A Textbook of Psychology,* 1909, 53, 340 ff.; Josephine Curtis, Duration and the temporal judgment, this JOURNAL, 27, 1916, 1-46; E. G. Boring, *The Physical Dimensions of Consciousness,* 1933, 19 f., 130 f.

[6] Hugo Münsterberg, *Beitrage zur experimentellen Psychologie,* 2, 1889, 20 f.

[7] J. C. Goudriaann, Le rythme psychique dans ses rapports avec les fréquences cardiaque et respiratoire, *Arch. néerl. de physiol.,* 6, 1921, 77-110.

[8] M. François, Contribution à l'étude du sens du temps: La température interne, comme facteur de variation de l'appréciation subjective des durées, *Année psychol.,* 28, 1927, 186-204; Hudson Hoagland, The physiological control of judgments of duration: Evidence for a chemical clock, *J. Gen. Psychol.,* 9, 1933, 267-287.

Passive Test. The first, the Passive Test, was given after the following instructions.

I am interested in learning how people tell time—how they judge intervals of time. I have here a bell—an ordinary dinner bell—I shall strike it once, then I shall strike it again. After the second stroke I wish you to tell me how long it felt to you between the two rings—how many seconds or how many minutes elapsed between them. It is not a question of getting the right answer, as in school; it is a question of how it naturally feels to you.

The bell was struck as soon as the instructions were comprehended, and after an interval of 45 sec., which was timed by a stop-watch, it was struck a second time. *S* was then asked, "Now, how long did that feel to you?" The reply was usually direct, expressed in terms of so many minutes. In every case the first estimate reported was recorded.

The purpose of the Passive Test was partly to determine variations in the evaluation of an arbitrarily chosen time-interval, and partly to acquaint *S* with the technique of the second test, the Active Test, which was to follow.

Active Test.—Immediately after the completion of the Passive Test, *S* was addressed again as follows.

All right, that's very good. Now this time I wish *you* to ring the bell, and to do it in this way. (Pushing bell toward patient.) Strike the bell once, then wait what seems to you *one minute* and then strike it again.

S then struck the bell, waited a subjective minute, and then struck it again. A stop-watch was used to measure the interval between strokes. The patient, irrespective of his result, was told that he had done very well, and was then asked how he had estimated the interval.

Results. The *S*s were divided into 6 groups according to their physical condition, and the temporal estimates were averaged separately for every group. Group 1 was composed of 20 *S*s presenting no clinical evidence of thyroid disease; Group 2, of 18 cases of thyroid adenoma without gross hyper- or hypofunction; Group 3, of 15 cases of similar thyroid adenomas at varying intervals following subtotal thyroidectomy; Group 4, of 10 cases of thyroid adenoma with basal metabolic rate of + 25% or more; Groups 5, of 45 post-operative cases of hyperthyroidism; and Group 6, of 17 cases of hypothyroidism with basal metabolic rate of —11% or less. For every group the average age in years, the average pulse rate, average basal metabolism, the average estimates of the temporal intervals in the two tests together with the ranges of the temporal estimates were calculated. The results are given in the accompanying table.

The most striking result is the wide range of variation in the *S*s' estimation of the length of the temporal intervals in both of the tests. The range, though not greatly differing among the diagnostic categories, was considerably larger in the Passive than in the Active Test. This result is probably due to the greater number of estimates in the Passive Test that were reported in 'minutes' as against 'seconds.' The smaller variability of the Active Test makes it more reliable in indicating trends.

It is also apparent that there was a tendency among the *S*s to overestimate the temporal-interval in the Passive Test, and to underestimate it in the Active Test. No correlation was found, however, between the amount of over- and underestimation in the two tests.

The non-thyroid cases, 20 in number, may be regarded as the control group. These *Ss* estimated the lapse of a minute on an average at 35 sec. Their 'subjective minute' was but slightly greater than half a 'clock minute.'[9]

The presence of a simple adenoma was associated with a still shorter 'subjective minute,' the average being 30 sec. (Group 2). The removal of the simple adenoma was associated with a slight fall to 27 sec. (Group 3).

In sharp contrast to the results of the simple adenoma group are those obtained from the hyperthyroid group (Group 4) which included both Graves' disease and adenoma with hyperthyroidism. The average estimate of this group was longer than that of any other, even the control. The lengthening of the 'subjective minute' in the group with the most rapid pulse and the highest basal metabolism does not support the view that the pulse or metabolism constitute an 'accelerated body clock.'

Group	No. Ss	Av. age	Av. pulse rate	Av. basal metab.	Passive Test (45 sec.) Av. est.	Passive Test (45 sec.) Range	Active Test (1 min.) Av. est.	Active Test (1 min.) Range
1. Non-thyroid	20	39	80	+2%	108	5–300	35	8–77
2. Simple adenoma	18	34	88	+6%	156	2–300	30	11–78
3. Simple adenoma post-operative	15	39	90	+6%	198	1–750	27	5–47
4. Metabolism of +25% or more	10	29	114	+39%	138	60–240	41	23–66
5. Hyperthyroid post-operative	45	39	93	+8%	126	1–300	31	5–73
6. Metabolism of −11% or less	17	31	80	−19%	120	5–450	40	4–80

With the pendulum of such a clock going faster than normal, one would expect on a priori grounds that the estimations would be shorter than normal. Such, however, is not the case.

Removal of the hyperactive thyroid resulted in a 25% drop (from an average of 41 sec. to one of 31 sec.) in the Active Test—almost as much below the control group as the hyperthyroid group was above it. The drop following subtotal thyroidectomy is in the same direction as the drop in the simple adenoma cases, but the degree is much greater.

The most surprising result of all is the similarity of the estimates of the two most widely contrasting clinical groups, *i.e.* Groups 4 and 6 which have respectively a basal metabolism of +25% or more and −11% or less, and an average pulse rate of 114 and 80. Group 4 gives an average estimate of 41 sec., and Group 6 of 40 sec. No fundamental antithesis in the 'time-consciousness' is demonstrated between the hyper- and hypothyroid *Ss*. It is conceivable that the 'time-consciousness' in these two groups is the expression of some factor common to both.[10]

[9] It is conceivable that there existed a conscious or physiological tendency to imitate the original arbitrarily chosen time-interval of 45 sec. that was given in the Passive Test. If so, the possibility was equally present for all *Ss*, and did not represent one of the variables of the experiment. The original time-interval might, however have been considered to have been a minute. Since it was really only 45 sec., their 'subjective minute' may have been shortened to conform to the misjudged standard.

[10] H. B. Richardson, The relation of the thyroid gland to Graves' disease, *Med. Clin. N. Amer.*, 18, 1934, 791-809.

In general no correlation is evident in either the Active or Passive Tests between the estimation of time and age, pulse rate, or basal metabolism, or with the effects of surgery.

Although the number of *Ss* employed in this study is admittedly small, the evidence at hand is against the view that bodily rhythms of pulse or metabolism as influenced by the thyroid gland from a basis of the temporal judgments. Introspections reported by the *Ss* indicate that what they could do in the given time was the criterion most frequently used in their estimations.

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THE TERM "AMBYSTOMA"

In the introductory section of Warren's valuable *Dictionary of Psychology* occurs the following passage: "In certain cases technical terms coined from Greek or Latin have been misspelled by certain writers, *e.g. ambystoma* for *amblystoma*, . . . In such cases we have preferred the correct etymological form.[1] In the vocabulary we find the entry, as promised:

> Amblystoma—a genus of salamander (Urodela).
> [Sometimes improperly spelled Ambystoma.][2]

Ambystoma, however, appears to be the correct form. As is well known, the most general principle determining biological nomenclature is priority; subject to minor restrictions, the name first published for a clearly described group is the authoritative name for that group. There is complete agreement among herpetological writers that the prior description of the group under consideration is that of Tschudi;[3] although the first adequate description appears on page 92 of the work cited, the following passage appears on page 57, thus eliminating the possibility of a purely typographical error:

> *Ambystoma* Tsch. . . . Das Genus Ambystoma (Salamandra subviolacea und venunosa Bart.) ist am nächsten mit unsern Salam. verwandt, aber doch hinlänglich von ihm unterschieden.

The formal description (page 92) is as follows:

> 3. *Ambystoma* Tsch.
> Caput magnum, convexum; parotides nullas; linguam mediocrem; dentes palatinos serie transversâ interruptâ, numerosos; digitos liberos; caudem teretem oblongam.
> Patria: America septentr.
> Ambystoma subviolaceum Tsch. [followed by synonymy]

There is thus no doubt that Tschudi is the legitimate author of the genus, that he named it *Ambystoma,* that he intended to name it *Ambystoma,* and that by the *general* rule of priority *Ambystoma* is the authoritative name. To this we may add that biographical reference shows that Tschudi, during his university days, was under the influence of philologists (which at this period implies classical philologists);[4]

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[1] *Op. cit.,* vii.
[2] *Ibid.,* 11.
[3] J. J. von Tschudi, Classification der Batrachier, *Mém. Soc. Sci. Nat. Neuchâtel,* 2, 1838, 57, 92.
[4] *Königliche Akademie der Wissenschaften; Allgemeine Deutsche Biographie.* 1875-1902.

so that the presumption is not only that he did name it and intended to name it *Ambystoma,* but also that he had some familiarity with the etymological considerations involved.

Suspicion of the acceptability of the form arose early, however, because of difficulty in ascertaining its etymology, which Tschudi unfortunately did not publish. The stem αμβλυ-, blunted, obtuse, weakened or diminished (cf. *amblyopia*) is a familiar one, and it was easy to see the rounded muzzle of the creature as blunted or obtuse in a geometrical sense. The stem necessary to account for *amby-*, on the other hand, is unusual, and evidently could not be found in available lexicons. Duméril and Bibron,[5] although using Tschudi's form, first took explicit cognizance of the discrepancy; subjacent to the caption of their section on the genus appears the curious entry, *Plagiodon. Nobis.*, with the footnote:

> De πλαγιος, transverse, en travers et de Οδους, Οδοντος, dents. Nous avions donné ce nom dans nos collections aux espèces réunies dans ce genre. Depuis, M. Tschudi l'a désigné sous un nom dont nous avons en vain recherché l'étymologie. Serait-ce Αμβλυστομα, bouche faible ou effacée? Nous l'adoptons, car il y a des Ophidiens Plagiodontes.

Hallowell, in his paper proposing the family,[6] significantly omits the derivation, although presenting it in almost every other comparable case. Strauch,[7] in a monograph in which the two names are almost impartially distributed throughout the text, formally treats the genus as "Amblystoma Tschudi," with the following footnote:

> Nach dem Beispiele Cope's nenne ich diese Gattung *Amblystoma,* von αμβλύ, stumpf, und στόμα, Mund, und bin überzeugt, dass Tschudi's fehlerhafte, aber ganz allgemein angenommene Schreibart *Ambystoma* nur durch einen Schreib- oder Druckfehler, an denen es in seiner Abhandlung bekenntlich nicht mangelt, entstanden ist.

With the data at hand Strauch, as well as Duméril and Bibron, would have been justified in "correcting" the apparent "Schreib- oder Druckfehler." The principle involved has been embodied in Article 19 of the International Rules,[8] which states that

> The original orthography of a name is to be preserved unless an error of transcription, a *lapsus calami,* or a typographical error is evident;

and a number of Opinions (*e.g.* Nos. 26, 27, 34, 41, 63) have shown the inclination of committees to correct obviously mistaken forms. Among these, Opinion 34 is most relevant to our case. It is to the effect that since evidence of the derivation of the word is not contained in the original publication, the original spelling *Aeshna* should be preserved in preference to the a priori more probable *Aeschna;* in other words, the author should be presumed to have warrant for his form unless there is evidence to the contrary.

The final and crucial question, then, concerns the etymological acceptability of the form which has been shown to have all the other technical requisites for authenticity. The most authoritative evidence may be sought in Liddell and Scott.[9] Here we find

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[5] A. M. C. Duméril and G. Bibron, *Erpétologie générale,* 9, 1854, 101.
[6] E. Hallowell. On the caducibranchiate urodele batrachians. *J. Acad. Nat. Sci. Phila.,* 3, 1855-8, 86-366.
[7] A. Strauch, Revision der Salamandriden-Gattungen. *Mém. Acad. Impér. Sci. St.-Pétersbourg,* 16, 1870, (no. 4), 60.
[8] Anon, International rules of zoological nomenclature, *Proc. Biol. Soc. Wash.,* 39, 1926, 75-104.
[9] H. G. Liddell and R. Scott, *A Greek-English Lexicon,* 1925, 78 f.

that the primary sense of ἄμβλυ- is *blunted, dulled, weakened,* and the sense *obtuse,* as applied to an angle, is plainly secondary; the one exception (among several dozens of citations), ἀμβλυχειλής "with rounded rim, of cupping-glass," would appear to represent some sort of confusion with ἄμβων, rim or edge of a cup. This latter word appears to be one of the group of cognates (to be considered) having reference to roundness of outline; it is either Attic or Rhodian, and has an Ionian equivalent ἄμβη, which seems to represent the minimum essentials of the stem we seek. The evidently closely allied word ἄμβιξ, a spouted cup, the cap of a still, or an alembic, is probably the form upon which Tschudi modeled his name; its inflected form ἄμβῑκος takes us naturally to βῖκος, jar or cask, drinking-bowl, or measure, and investigation of the other syllable indicates that the fundamental sense of ἄμᾰ (shortened to ἄμ in composition) is *together with*; thus we may suppose that ἄμβιξ was primarily the lip which characteristically accompanied the cup, and finally the lipped cup itself.

There seems to be no escape from the conclusion that Tschudi's form embodies a compositional inelegance; for the smoothest form for the derivative from ἄμβον would be *Ambonostoma*, that from ἄμβιξ would be *Ambikostoma*, and that from the Ionian ἄμβη would be *Ambestoma*. A reasonable account of the actual formation would seem to be that he proceeded from the nominative ἄμβιξ, without taking account of the stem, and thus arrived at *Ambyxstoma* as a first approximation, from which point the most direct route to euphony would be the elimination of the *x*. In any event, there is every reason to believe that the form was deliberate and referred to the *rounded* mouth rather than to a dulled or weakened mouth, so that short of a special Opinion (the case for which would seem to be weak) the form must stand.

A brief synopsis of the usage may be of interest. The form was named in 1838, and the first papers which include the name and of which we can find trace thereafter are those of Baird;[10] while writing alone, Baird adhered to Tschudi's form, but his paper with Girard in 1852-3 goes over,[11] without explicit comment, to *Amblystoma,* to which Baird thereafter adhered.[12] We have already alluded to the doubts of Duméril and Bibron, who adhered to Tschudi, and of Strauch, who did not. Hallowell[13] and Gray[14] consistently wrote *Ambystoma,* but Cope,[15] Boulenger,[16] and

[10] Summarized in S. F. Baird, Revision of the North American Tailed-Batrachia, with descriptions of new genera and species, *J. Acad. Nat. Sci. Phila.*, 1, 1846-50, 281-294.

[11] S. F. Baird and C. Girard, Characteristics of some new reptiles in the museum of the Smithsonsian Institution, *Proc. Acad. Nat. Sci. Phila.*, 6, 1852-3, 173-177.

[12] *E.g.* S. F. Baird and E. D. Cope, Discussion, *Proc. Acad. Nat. Sci. Phila.*, 14, 1861, 123-124.

[13] E. Hallowell, Description of several new North American reptiles. *Proc. Acad. Nat. Sci. Phila.*, 10, 1857, 215-216.

[14] J. E. Gray, Catalogue of the specimens of Amphibia in the collection of the British Museum, Part II, British Museum, 1850.

[15] E. D. Cope, On the structures and distribution of the genera of the arciferous Anura. *J. Acad. Nat. Sci. Phila.*, 2 (2nd ser.), 1866-9, 67-112; A review of the species of the Ambystomidae. *Proc. Acad. Nat. Sci. Phila.*, 20, 1867, 166-211; Batrachia of North America. *Bull. U. S. Nat. Mus.*, 1889 (no. 34).

[16] G. A. Boulenger, *Catalogue of the Batrachia gradientia sive Caudata and Batrachia apoda in the collection of the British Museum.* (2nd ed.) British Museum, 1882.

the Cambridge Natural History as consistently preferred *Amblystoma.*[17] In modern times there is perhaps a slight majority of preference for the latter, but Pratt[18] and Jordan[19] are correct; the latter gives a correct etymology and notes that *Amblystoma* is incorrect, but without documentation. Psychologists, probably following Coghill[20] and Carmichael[21] have been virtually unanimous in writing *Amblystoma.* The recent edition of Webster's New International Dictionary is not wholly clear;[22] the word is listed as *Ambystoma,* but the derivation is given as "[NL. (sic); fr. *amby-* (for *ambly-* + *-stoma.*]," and the comment is appended: "Commonly called *Amblystoma,* although originally *Ambystoma.*" *Biological Abstracts* appears to favor *Ambystoma.*[23] In view of the evidence presented above, we hope that the psychologists will presently follow suit.

Clark University RAYMOND ROYCE WILLOUGHBY

A NOTE ON THE RELATION OF THE DOMINANT THUMB IN CLASPING TO HANDEDNESS

The uppermost thumb when the hands are clasped together has frequently been accepted as an indication of latent handedness tendencies. Adler has written, "the left handed subject will *instinctively* do this so that the left thumb is over the right thumb."[1] In 1908, however, Lutz had called attention to the fact that while the same thumb is for any given individual usually outside when the hands are clasped, on the basis of his results from approximately 600 individuals he was forced to conclude that the position of the thumb had apparently no relation to right or left handedness.[2] Since that time others have accepted and used the test; Cuff[3] and Scheidemann[4] have combined it in their batteries of hand preference tests, and Haefner[5] also used it, showing that 53% of his right-handed children clasped with the right thumb up while only 35% of the left-handed children did so. June Downey,[6] who reported the earlier view of Lutz, denies his claim and, presenting percentage differences very similar to those of Haefner, concludes that there is a significant dif-

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[17] H. Gadow, Amphibia and reptiles, *Cambridge Natural History,* 8, 1901, 109 ff.
[18] H. S. Pratt, *A Manual of the Land and Fresh Water Vertebrate Animals of the United States,* 1923.
[19] D. S. Jordan, *Manual of the Vertebrate Animals of the Northeastern United States inclusive of Marine Species,* 1929.
[20] G. E. Coghill, *Anatomy and the Problem of Behavior,* 1929.
[21] L. Carmichael, The development of behavior in vertebrates experimentally removed from the influence of external stimulation. *Psychol. Rev.,* 33, 1926, 51-58.
[22] W. A. Neilson [Ed.], *Webster's New International Dictionary of the English Language,* 1934, 82.
[23] *Op. cit.,* 8, 1934, 2473, 7525.
[1] Alfred Adler, *Problems of Neurosis,* 1929, 114.
[2] F. E. Lutz, Inheritance of the manner of clasping the hands, *Amer. Natur.,* 42, 1908, 195.
[3] N. B. Cuff, Study of eyedness and handedness, *J. Exper. Psychol.,* 14, 1931, 327-329.
[4] N. V. Scheidemann, Study of the handedness of some left-handed writers, *Ped. Sem.,* 38, 1930, 510-516.
[5] Ralph Haefner, Relation between hand and foot tendencies of children, *ibid.,* 338-350.
[6] June Downey, Further observations on the manner of clasping, *Amer. Natur.,* 60, 1926, 387-391.

ference, so that clasping the hands with the left thumb outside points to latent sinistral tendencies.

In the present study, in which there were 75 Ss, 49 of them clasped the hands with the left thumb uppermost and only 26 with the right thumb on top; in every case the thumb on top was consistent in four trials. These results alone suggest a much greater prevalence of sinistral tendencies than has ever been indicated. Four other handedness tests were, however, used: writing and erasing, throwing, dotting, and gripping strength with the hand dynamometer. These tests were scored so that total handedness scores might be derived ranging from 4 representing entirely consistent right handedness, to —4 representing entirely consistent left handedness, with intermediate scores representing varying degrees of consistency. The handedness scores of the two groups were as follows:

Clasping with right thumb up		Clasping with left thumb up	
Handedness score	No. Ss	Handedness score	No. Ss
4	10	4	18
3	11	3	24
2	2	2	4
1	1	1	1
0	0	0	1
—1	0	—1	1
—2	1		
—3	1		
Av. handedness score	2.8	Av. handedness score	3.1

Although it would seem that those individuals clasping the hands with the left thumb uppermost are more right-handed than those clasping with the right thumb on top, the differences are not reliable, but they present striking evidence that in this group there is absolutely no relation between the dominant thumb in clasping the hands and handedness tendencies.

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BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Problem of Mental Disorder. A study undertaken by the Committee on Psychiatric Investigations, National Research Council, Madison Bentley, chairman, and E. V. Cowdry. New York, McGraw-Hill Book Co., 1934. Pp. x, 388.

It is widely acknowledged that the mental disorders are poorly understood and very inadequately controlled. The Committee on Psychiatric Investigations was formed with the central interest "to discover how well the natural sciences are prepared to form a basis of support, in the way of fact, method, and principle, for the psychiatrical arts and for a sound doctrine of the relation of the abnormal to the normal in human activity and behavior." *The Problem of Mental Disorder* is the primary outcome. In view of the increased social and economic importance of the problem, this inventory of the available scientific support is a timely contribution.

The exposition is divided into four sections. The first outlines the character of the problem. The second section contains five psychiatrical points of view; (1) *clinical psychiatry* by Dr. Campbell, (2) *medical psychiatry* by Dr. Myerson, (3) *the neurologist's point of view* by Dr. Wechsler, (4) the *psychobiological point of view* by Dr. Meyer, and (5) *the psychoanalyst's point of view* by Dr. Kubie. This section provides a comparison and an orientation in the principal schools of psychiatry. The emphases in theory and method differ, but each makes at least some acknowledgment to the theses of the others. Most nearly opposite are the views of Myerson and Kubie. Meyerson, considering paresis the paradigm for all of the disorders, puts his faith in the concept of disease and would practically limit investigation to the pathology underlying the disorders. But even he is prepared to believe "that chronic emotional states may produce pathological alteration." Dr. Kubie, as a psychoanalyst, emphasizes the etiological importance of the individual's history, biographical and biological, and stresses the diagnostical and therapeutical value of the method of free association. But he acknowledges limits "to the realm in which psychogenic forces play the dominant rôle." The other three expositions represent variations of emphasis between these two extremes. Dr. Meyer is especially anxious to take every factor into account which may be pertinent, treating each person as an individual. Each of these men has his own terminology and favorite techniques which suggest individual training and personal philosophy more than factual differences.

The third section (275 pages), consisting of twenty papers describing present contributions and future research from the supporting sciences, constitutes the major portion of the volume. These twenty papers may be divided into five groups: (1) the first eight dealing with nervous mechanisms and functions, (2) a group of two describing constitutional and hereditary factors involved in the disorders, (3) four papers devoted respectively to endocrinology, virus diseases, nutrition, and pharmacology, (4) three articles devoted separately to experimental, clinical, and comparative psychology, and (5) the three final articles which examine cultural, racial, and educational factors influencing the psychological manifestations of disorder.

Reading the papers devoted to the neurological and physiological sciences, one is struck anew with the great gaps in our knowledge. Considerable new knowledge,

not to be found in text-books of neurology and psychiatry, is sketched. New and hopeful lines of investigation are outlined. Yet a long road of research has still to be travelled before the relations of the complicated psychological operations of the organism to underlying morphological structures and physiological functions are to be made clear. Such fields as the electrophysiology of the brain, neurocytology, and the chemistry and metabolism of the brain appear to have fine future possibilities, but they offer little that is immediately applicable to the problem of mental disorder. Dr. Jacobson's electrical measurement of activities in nerve and muscle is exhibiting immediate promise. While there are many open problems in the field of endocrinology, it too appears ready to make immediate contributions.

Psychology, like these other fields, has too little to offer at the present. Fruitful avenues of research are indicated, however, especially (as the reviewer believes) by way of the genetic approach through long-term studies of individuals, the psychological study of children, and the comparative psychopathology of infrahuman primates. The cultural sciences should also make a large contribution in the future. They deserve more space than they were allowed in this study, which tried to limit itself to the natural sciences.

No one can read these twenty papers without being impressed with the wide ramifications of the problem of disorder. As too many workers in psychiatry appear to be carried away by a single approach, the bringing of these ramifications into clear relief is a major contribution of the Committee's study. Another major contribution is the appeal to descriptive facts in a field rife with speculative theories. A criticism might be leveled against some of the contributors, however, for not being more explicit in their suggestions for future investigation.

In the fourth and final section, *comments and reflections,* Dr. Bentley has pointed out that "the scientific materials at hand considerably exceed the actual use thus far made of them in psychiatry." He suggests also that studies should be made of such 'part-problems' as the comparative value of our existing systems of care and treatment. The book should be read by everyone interested in any way in the problem of mental disorder. Moreover, it should provide a new stimulus for coöperative research in this complex and difficult field.

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The Fundamentals of Learning. By E. L. THORNDIKE et al. New York, Bureau of Publications, Teachers College, Columbia University, 1932. Pp. xvii, 638.

This book presents in detail the results of extensive research into the basis of learning, first outlined briefly in the *Proceedings of the National Academy of Sciences,* 13, 1927, 15-18, and published in a less complete form than in the present volume in *Human Learning,* 1931. The work was made possible by a grant from the Carnegie Corporation. The question of effects of frequency of stimulation is attacked as follows: If situation S_1 brings about response R_1 16 times, R_2 3 times, and R_3 once, will the next 20 occurrences of that situation increase R_1 and lessen R_2 and R_3 because of the greater frequency of R_1 in the first series? In one experiment, for instance, S_1 is a strip of a given length, and R_1, R_2, R_3, etc. are estimates of its length. If frequency is an effective principle in itself the connection between stimulus and response $S_1 \rightarrow R_1$ would be strengthened in the second series and connections $S_1 \rightarrow R_2$ and $S_1 \rightarrow R_3$ weakened. No such results of the effects of the greater initial practice were found. Similar negative results were obtained in experiments directing

the *S*s (eyes closed) to draw lines of a given length, as 2, 4, 6, or 8 in.; and also in those directing them to draw angles of 20°, 45°, or 60°. The net outcome of all these experiments was to show no efficacy in the mere frequency of repetition of a situation. It was also found that if 150 words, each followed by some number from 1 to 9, have been read to the *S*s, and if the *S*s then hear 960 words read at the rate of one every 2½ sec. and have been instructed to write a number from 1 to 9 after hearing each word, the greater frequency of certain connections in the earlier readings did not result in increased frequencies of connections of these elements in responses written later (p. 43). In these experiments the results of certain subjects were discarded entirely because "it seemed certain that their responses were determined by causes irrelevant to our problem" (p. 34); for example, an *S* might repeat a single number, like 4.

In other experiments the *S*s were required to respond to certain sounds of letters and letter combinations (*e.g. ow* as in *house, aw* as in *lawn, k* in *Kansas, s* in *so,* etc.) by writing the letters representing them; and to respond to certain visually presented 2- or 3-letter groups (*bl, cha,* etc.) by writing the first word thought of in each case which begins with the letters given (*e.g. black, charade,* etc.). In this latter experiment, we are told, "we see the power of satisfyingness compared with that of repetition. The ease of writing a short completion and the satisfaction of finishing a sheet quickly in the presence of others doing the same task put a premium upon short completions, although there was no formal requirement of speed" (52 f.). Here an interpretation enters. The "satisfaction" seems not to have been reported by the *S*s but inferred by the experimenter; but why is satisfaction used for explanation of these differences, when the responses themselves are different (even if the inference of satisfaction is correct)? Thorndike does not show how this satisfaction operates. It is only a hypothesis, and an untestable one at that, since the satisfaction, if it actually exists, is regularly accompanied by other observable conditions—the ease and speed of writing the shorter words. Responses, through proprioceptive and other stimuli, modify subsequent responses.

Additional experiments, too complex for consideration here, and too much intermixed with controversy to be described clearly in short space, relate to effects of "belongingness" (which sometimes seems to mean mere frequency of previous connections, as in habitual groupings or classifications, at other times coördinate relationship, the consistency of elements with one another, etc.); to the influence of impressiveness, of distribution, of repetition of a series on the omission of its intermediate terms; and to polarity in mental connections. They are well worth careful study. Most of them are not as well controlled as are the first experiments on frequency effects. In some experiments the spoken words *Right* or *Wrong,* according to the success of the *S*s in estimating lengths, drawing lines, etc., resulted in definite improvement in successive trials. The interpretation of the author, that this improvement was due to the "satisfying connections" (instead of the extra stimuli thus afforded), is, of course, questionable, and has given rise to much discussion in psychological and other journals. Numerous experiments have shown that immediate and accurate checks of the learner's efforts greatly expedite the learning. Moreover, the effectiveness of pleasure is not established even if hearing *Right* is pleasurable (but cf. p. 194). Twenty-seven years ago Thorndike, probably in an unguarded statement, said of the animal learning a problem box: "But if it does hit upon the proper act, the resulting pleasure stamps in that fragment of its mass of instinctive

impulses and renders it more likely to come up in connection with the sense-impression of the box. The failure of all the other acts to win pleasure stamps them out" (*Psychol. Rev.*, 5, 1898, 551). Since then he has come to use "satisfiers" and "annoyers" instead of pleasure directly in the stamping-in and stamping-out functions, but these terms themselves are not explanations.

In multiple-choice types of learning, Thorndike found that speaking the word *Right* for success was much more effective than saying *Wrong* for an error. These results are, of course, to be expected, since only one response could be correct and four of the five possible ones could be wrong, so that each call of *Right* definitely locates and emphasizes the correct response, while the call of *Wrong* does neither of these but may even produce a confusion of impulses. It gives no *direct* 'set' toward the right response, but only a probability of 1 to 4. Hence punishment may be expected in such problems and in others roughly like them, as problem-box learning, to be less effective than reward (cf., however, the discussion in Ch. 11). Various experiments on animals in alternative types of choice have demonstrated greater influence of punishment than of rewards. There are, however, several good reasons why human beings, especially adults, may profit much more from rewards in most sorts of circumstances than animals do. This fact Thorndike recognizes (p. 455). The considerations here mentioned should have more attention in the chapter on rewards and punishments than they receive.

There are other chapters, including some experiments, on The Physiology of After Effects; Readiness, Identifiability, and Availability; The Influence of Mental Systems; Desires, Purposes, Interests, and Motives; Associative Shifting and the Conditioned Reflex; Minor Experiments, Comments, and Suggestions for Further Investigations; and Adverse Evidence and Arguments—all of which are well worth reading. Ten appendices of 144 pages contain valuable materials on apparatus and forms used in the experiments, tables of data, etc. These are followed by over six pages of references and a rather inadequate index of subjects and names.

Thorndike has been one of the last of American research psychologists to give up confidence in the effects of mere frequency of stimulation in learning, despite the fact that in 1911 he gave the death blow to a frequency theory proposed in 1908 by Stevenson Smith; and the literature of education bears witness to the extent that his earlier writings have influenced educational practice. His recent studies are therefore most fortunate since his influence on educators will enable him effectively to turn the tide against earlier errors. It is somewhat amazing that Thorndike should write up these experiments without any consideration of, or reference to, most of the experiments by other psychologists which had demonstrated the futility of mere repetition in learning which preceded by several years his own experiments. Some of these pioneer experiments were published exactly ten years before the first announcement of his own results, and showed the ineffectiveness in constructive learning of both frequency and recency factors as such. Experimental psychologists were therefore not surprised at his results. His work, however, is of much importance both as corroborative of earlier results and as corrective of much mere repetitive work in education for which he is to a considerable extent responsible. His "law of effect" is not unacceptable to other pschologists if he will interpret it in straightforward, objective terms without falling back on hedonistic elements and non-testable assumptions of the operation of "satisfiers" and "annoyers."

While the experiments presented in this volume are in most cases not models of

experimental technique, they deal with practical situations (found mostly, it is true, in passive, associative learning and drill work in the schools) and they show the thoroughness and persistence of their author and director.

J. P.

Children's Sleep. By S. RENSHAW, V. L. MILLER, and D. P. MARQUIS. With an Introduction by W. W. Charters. New York, Macmillan Co., 1933. Pp. xix, 242.

The authors studied 79 girls and 92 boys, all being inmates of an institution. Their ages ranged between 5 and 20 years, through a middle range of 10.3 to 15 years, and a median of 12.8 years. Their so-called intelligence-quotients ranged between 30 and 120, through a middle range of 79 to 99, and a median of 89. The authors sought for a typical picture of the nocturnal "motility" of the sleepers, and of its changes with certain systematic variables, one of which was certain motion-pictures presented in the evening.

First, *they detected each minute during the night in which the sleeper's bodily movements actuated the pen of a kymograph.* The pen was controlled by an electro-magnet in the circuit through a stationary brush and a movable commutator-bar attached to the bed-spring so as to be displaced vertically by the sleeper's movements. Each segment of the bar was 3.0 mm. wide, and all the conducting segments were in parallel. In certain critical positions of the brush on the bar, an infinitesimal displacement in one direction would excite the pen, whereas a displacement of 5.5 mm. in the other direction was required. They wished to record all gross changes, and only gross changes, in the sleeper's bodily position, but evidently they recorded some very slight changes also. The extent of travel of the bar does not correspond either to the magnitude or to the frequency of displacement of the pen; hence, the record does not distinguish gross movements from slight movements of the sleeper.

All the beds appear to have been alike, the mattress resting on a system of cables fastened at their ends to the frame through helical springs. Each cable was clamped rigidly at a few points to either of its nearest neighbors, and to no others. The commutator-bar was attached to the central cable, near its middle: hence, a person, in making a given movement, will displace the bar farther if he lies near the center of the bed than if he lies near its edge. Thus, there is a critical class of movements, any of which will displace the pen if the subject occupies any of a certain class of locations in the bed; not if he occupies any location not belonging to this class. Hence, in the long run, the larger and heavier the sleeper is, the more of his movements will be registered. Moreover, if the class of children between 14½ and 18 years of age contains relatively more large and heavy sleepers than the class of 10½ to 14½ year-olds, and relatively still more than the class of 5 to 10½ year-olds, we should expect the proportion which the number of detected movements bears to the number of actual movements to increase according to the age-range of the several classes of sleepers. Now the authors *found* that the older subjects *registered* movement more frequently than the younger subjects. They *assumed* this finding to mean that the older subjects not only registered, but also made, more movements than the younger. They left this assumption untested. I question its factual truth.

Second: the authors counted the active minutes in each hour, and presented the count separately for each hour of the night. If the same subject slept many nights under comparable conditions, they averaged the hourly count night by night; if

many subjects in one-age group slept under comparable conditions, they averaged the averages of their individual subjects. On the latter procedure they base an important interpretation, although on p. 71 they first deny that the procedure can be legitimately applied to their results.

In their most typical experiment (V) they report (p. 134) that on movie-nights, in the average, the boys stirred in 1.24 *more* minutes per hour, and the girls in 0.2 *fewer* minutes per hour, than on normal nights. They present these differences along with determinations of their standard errors, neither standard error being computed correctly from the data in the table. The difference returned by the boys is actually 1.18 (not "3.786") times its own standard error, while the difference returned by the girls is 0.018 (not 0.015) times its own standard error. The authors discuss the difference found for the boys, and also the difference between it and the difference returned by the girls, as if the boys' difference were statistically reliable, whereas it is properly attributable to chance. If it were reliable, it would signify that "the average boy," spending nine hours a night in bed, stirs in 11 more minutes of that time on movie-nights than on normal nights. What kind of good, and how much of it, would "the average boy" have got out of these 11 minutes if he had spent them in rest? I wonder!

The authors compare these differences with those which *other groups of subjects* return, between their average motility on odd-numbered and even-numbered nights, and between their averages based on many normal nights and their averages based on a few holiday-nights. None of the differences however is significant.

In the same experiment it appears from Table 31, p. 125, that "the average boy" on movie-nights is much more active in the first hour, and slightly less active in the 7th and 8th hours of the five movie nights than in the corresponding hours of 35 normal nights. In other words, on the movie nights, the sleeper postpones his settling down to rest, and rests only a little longer at a time in the later hours than he usually does. But Garvey and also Johnson and Weigand found that this is a perfectly normal occurrence. From the tables of the present authors, pp. 126 ff., it appears that the weighted averages of the averages of the individual subjects' active minutes per hour are subject to relatively large standard deviations, as are the corresponding averages of the individual subjects. Could one then choose five normal nights, which stand in the same temporal order as these five movie-nights, the records of which (by the Chi-squared criterion) would differ from the normal as widely as do the records of these five movie nights? Before we can test the authors' appraisal of the influence of the movies, we need to see a just account of negative instances; and this remains to be presented.

The authors give (p. 153) some contradictory interpretations of the results. In Conclusion (1) they say that the movies were followed by changes in motility that cannot be attributed to normal variations, or to the "holiday-effect." This conclusion contradicts the data presented. In Conclusion (2) they say that a *decrease* (as well as an increase) of motility from the normal *diminishes* the "recuperative effects" of sleep; in Conclusion (3) they say that *decreases* in motility *enhance,* while *increases lessen,* these "recuperative effects." At least one of these assertions must be false. Again, on p. 99 they say and on p. 154 (Conclusion 5) they imply, that the greatest "recuperation" occurs in those hours in which the normal subject is usually the least motile: namely, his first two or three hours in bed. These assertions are not logically supported by any valid tests of "the recuperative effects of sleep" that

are cited by the authors or known to me. Indeed, I have not yet seen exhibited a set of feasible operations that could define such a test.

Thus, certain parts of the verbal description contradict the authors' statistical exhibit. The statistics themselves contain some incongruities. Figures 12 f., pp. 80 f. rank the seasons in different orders according to the average motilities of the different classes of subjects, although Fig. 12 purports to have been derived from Table 13. Also, the extrapolations in Fig. 12 contradict some facts. Again, Tables 16 f. pp. 100 f. are mislabelled. Their abscissae should read "(M/N — 1) per cent," not "MN/ %." Fig. 28, p. 150, showing the relation between the mean length of the quiet periods to the number of active minutes per hour, does not show a restriction of the field of the equation given on p. 149. This restriction prevents the equation from asserting that if a child should lie still as long as an hour he would never stir again. But it also limits the field of the equation to intervals of exactly one hour, each of which ends in an active minute. But a generalized formula exists, which gives the same values as this one gives within the same field; which does not presuppose that a sleeper cannot lie still as long as one hour; and which is applicable to any temporal range that ends with the beginning of an active interval, whatever the unit-interval may be. It is the formula of Johnson and Weigand, cleared of the assumption that half of each unit-interval in which movement occurs is passive. Were the *original* formula of these authors, and the formula of the present authors, applied to the same records, they would yield two estimates of the mean rest-period which would differ by exactly half of one unit-interval: in this instance, by half a minute. The present authors say that the use of a 5-minute unit-interval instead of a unit-interval of one minute, "distorts" the exhibit. It remains to be shown how great this distortion is.

In summary: there is no doubt that the authors have collected a set of facts that have great potential value. Their presentation, however, is faulty. The comparison of average motilities among groups of different age, and in general, between any two individuals who differ greatly in either weight or stature is invalid. The influences of systematic variables on the motility of the same sleeper can be determined from the records, but the authors have used a set of statistical forms that do not exhibit the differences well. The historical sections of the book are especially poor. The historian discusses certain studies with which he seems to be unacquainted, since his account contradicts the original report. He sometimes quotes with approval the remarks of certain other historians who depend on secondary accounts written by authors who have not read the original reports and who accordingly misrepresent them.

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Remembering. By F. C. BARTLETT. Cambridge, University Press, 1932. Pp. x, 317.

Starting from a conventional criticism of the shortcomings of Ebbinghaus' nonsense syllables as materials for the study of remembering, the author proposes to substitute a methodology which will permit the study of remembering in terms of functions and their conditions. The final value of the book depends not so much upon any novelty of analysis or upon the creation of new methods of investigation, but rather upon the demonstration of how prolonged and thoughtful research with

extremely simple methods and everyday materials may be made to yield significant generalizations.

Since the author insists that "remembering is not a completely independent function, entirely distinct from perceiving, imaging, or even from constructive thinking, but has intimate relations with them all," certain allied accomplishments of the organism are treated in support of the investigation of remembering. The latter is, however, always the main concern of the volume, and will be the sole topic of discussion in this review. The reader will readily recognize three divisions in Bartlett's exposition: first, a discussion of the results of his own experiments; second, a theoretical section; and third, a brief presentation of Bartlett's investigation of remembering as a social phenomenon. Each of these may be considered in turn.

A section on the Method of Description deals with the first of Bartlett's researches. These were carried out during the war and appropriately began with the exposure for ten seconds of picture postcards, representing five common types of men in the armed forces of Great Britain. After intervals of thirty minutes, and also of one or two weeks, the twenty subjects were quizzed about the pictures. Results are given in some detail, but aside from noting that remembered objects are shown to be 'transformed' by method of recall and by feelings or interests, we may summarize Bartlett's findings in his statement that "even when material is arranged in a short series, is small in bulk and simple in objective structure, and when it is so given that an observer knows that he will be asked to describe it later, remembering is rapidly affected by unwitting transformations: accurate recall is the exception and not the rule."

The Method of Repeated Reproduction is concerned primarily with the effects of having the same individual continue to repeat a story after various lapses of time and noting the changes that occur. The section dealing with graphic materials stems from Philippe's familiar work of the 90's and obtains comparable results. The verbal material in the other section of this chapter involves the presentation of an unusual story and the consideration of its fate as it is repeated over intervals which vary from a few weeks to ten years in duration. Results point to a persistence of form or outline, once the first version has been reproduced, but with little persistence of the style or mode of construction of the original. The tendency towards stereotypy which Philippe reported is again evident here, following rapidly upon the first reproductions. "In all successive remembering, rationalization, the reduction of material to a form that can be readily and 'satisfyingly' dealt with is very prominent."

The Method of Serial Reproduction is similar to the method just discussed, except that the succeeding reproductions are now carried on by different individuals—a refinement of the familiar classroom demonstration in which each individual repeats a story to his neighbor after it has been relayed to him. "In every single case . . . the final result, after comparatively few reproductions, would hardly ever be connected with the original by any person who had no access to some intermediate versions." "Epithets are changed into their opposites; incidents and events are transposed; names and numbers rarely survive intact for more than a few reproductions; opinions and conclusions are reversed—nearly every possible variation seems as if it can take place, even in a relatively short series. At the same time, the subjects may be very well satisfied with their efforts, believing themselves to have passed on all important features with little or no change." When drawings are substituted for

stories, similar conditions are found to prevail, with the usual tendencies to simplification, conventionalization, and elaboration.

The Method of Picture-Writing undertakes to show what happens when observers are required to learn a vocabulary of picture-signs and are then asked to use these signs later in writing from dictation. The types of changes described above are noted, and here, as elsewhere in the experiments, a clear difference is shown to exist between those who remember primarily in visual terms and those who remember chiefly by naming all or part of the object originally shown. A strong influence of naming is found consistently, with a general tendency for those who have relied upon names to be less confident in their recalls.

The concluding section of the volume deals with 'Remembering as a Study in Social Psychology,' reporting the author's attempts to make profitable use of a visit to Swaziland, South Africa. One is tempted to summarize his results by quoting his statement that " . . . the group itself, as an organized unit, has to be treated as a veritable condition of human reaction." Or perhaps a better summary is his thesis that " . . . the matter of recall is mainly a question of interest, while the manner of recall is chiefly one of temperament and character." Illustrations are given by citation of the performances of the natives in remembering items for Bartlett and in their recall of folklore and the like. He finds a "low level type of recall which comes as nearly as possible to what is often called rote recapitulation. It is characteristic of a mental life having relatively few interests, all somewhat concrete in character, and no one of which is dominant." On the other hand, "whenever strong, preferred, persistent, social tendencies are subjected to any form of forcible social control, . . . social remembering is very apt to take on a constructive and inventive character, either wittingly or unwittingly." On this basis, the reader will not be surprised to find Bartlett discarding as unsupported and unnecessary the theory of a collective unconscious.

Although Bartlett ends his volume with the sections on social psychology, it may be more appropriate to reserve for the final sentences of the review a consideration of his theoretical contributions. His theories must be interpreted in the light of his credo that "perceiving, recognizing, recalling are all psychological functions which belong to the same general series." Holding that "the past operates as an organized mass rather than as a group of elements," Bartlett differentiates between recognizing and remembering by describing the former as "reaction *by means of* organized psychological material" while remembering is *"reaction to* organized psychological material." In remembering he finds that the organism 'turns around upon its own schemata' (= traces) to construct them afresh—a process which "gives consciousness its most prominent function." This turning around upon schemata, the author of the volume confesses regretfully, he cannot understand.

The volume holds throughout the impress of an author who seeks thoroughly to interpret a few carefully gathered data in search of broad conclusions, rather than to scrutinize minutiæ in a problem dimly understood. It is, however, the work of a man living in apparent intellectual isolation—a man who makes no effort to align his results with the great mass of existing experimental information —a man who seems unaware of the extent to which he is using conventional methods of measuring remembering. There is no evidence that he has searched the literature for the work of his contemporaries; if he is aware of pertinent researches by Whipple, Perkins, Gibson, McGeoch, Stern, Wulf, or Granit, there is nothing to indicate the fact. Even his reference to the classic work of Philippe is apparently

due to the fact that his "attention was drawn to Philippe's most interesting work by the late Prof. James Ward."

This disregard of the researches produced outside the borders of the British Empire and the complete failure to envisage his own work as part of a larger total have resulted in the production of a book which might be classified as the work of a 'naturalist' (to borrow a term from biological contexts). While the isolation of the author adds a certain freshness to his researches, it very greatly reduces the value of his theoretical contributions and of the volume as a whole. The book will find a place upon the shelves of those who study remembering, but it will not be in the special section reserved for those investigators whose writings have become landmarks in the advance towards the comprehension of this important problem.

Cornell University JOHN G. JENKINS

The Adolescent in the Family: A Study of Personality Development in the Home Environment. Report of the Sub-Committee on the Function of Home Activities in the Education of the Child. E. W. Burgess, Chairman. White House Conference on Child Health and Protection. Section III. Education and Training. New York, Appleton-Century Co., 1934. Pp. xviii, 473.

This is an extensive statistical study of the home life of adolescents. The subjects were about 13,000 children of Junior High School age belonging to several nationality groups and living in rural, small town, and urban centers of population in various portions of the United States. The method used is that of the questionnaire, supplemented by teacher's reports and narratives of home life by about 300 college students.

The questions for the most part were answered by check marks, numbers, or the words "yes" or "no," and the answers involved memory of objective events rather than introspective judgments. They usually referred to a definite period of time, for example, "how often did you go to the movies last week?" "Has your father been sick in bed during the last year? How many days?" "Were you punished at home last week?" But in a personality questionnaire less specific questions were asked, such as "Do people find fault with you much?" "Are you happy most of the time?"

The discussion and tables occupy 236 pp.; illustrations of types of families quoted from papers of college students, 26 pp.; discussion of family relations and personality development, 37 pp.; and recommendations, 4 pp. In the appendices 85 pp. are devoted to tables and 64 to description of methods and the blanks used, while the well arranged index covers 9 pp. The teacher's schedules call for numerous facts regarding children and their homes with rating scales to be checked. The narratives of college students were used chiefly to illustrate types of famiies implied by the statistics of pupil and teacher schedules.

In the nature of the case questionnaires can never have the reliability and accuracy of laboratory experiments, but when the questions are significant, easily and honestly answered, objective in character, and in form to admit of statistical treatment as these are, they have a considerable value. Many of the facts revealed have been known by some observers, but the statistical generalizations and comparisons are much more reliable and accurate than personal judgments of single individuals.

Much credit is due to the research assistant, Dr. Ruth Shonle Caven, who wrote the report. The tables in the body of the work and in the appendix are accurately

labeled, well arranged, ably summarized, interpreted and compared with each other and with the results of other researches. The reader is given a clear idea of different grades of homes according to financial, social and cultural status, as found in urban and rural sections in general, and among Negroes, Germans, Jews, and Italians. The effects of these objective factors on personality development as compared with the social relations existing within the families, are found to be less potent than one might expect. Some of the most important conclusions are that confidential relations with parents, freedom from disapproval and punishment, are more favorable to a well adjusted personality than the wealth of a family, its social status and surroundings, place of the child in the family, as oldest, youngest, only child, mother employed or unemployed, or occupation of father. Although it is found as in other researches, that more delinquents and more poorly adjusted children are members of broken homes, yet the evidence shows that a disorganized home is more surely productive of poor personality development than one broken either by death or separation. The most significant facts regarding parents is whether or not they are nervous.

From data gained in this study, a rating scale of factors influential in personality development was formed which shows with considerable probability whether a child is likely to have a disorganized or a well organized personality. With further improvement in this scale it may become nearly as valuable in studying health of personality as intelligence tests are in indicating grades of intelligence. Other studies show that intelligence is closely associated with family ability, as indicated by economic, social and occupational status, while this study indicates that personality organization depends upon family functioning, whatever its status may be.

The authors, unlike many persons dealing with statistical evidence of abnormality, recognize to some extent, though perhaps not sufficiently, that in the scale of distribution a score near the average in most factors is likely to be a better indication of normal health than scores at either extreme of the scale. Probably they recognized this fact in formulating the question regarding confiding in parents. The answers to be checked were: "Almost never," "sometimes," and "almost always." Always to confide in mother would probably be more unfavorable to healthy personality than almost always to do so. The book is an important contribution to the literature of adolescence.

Leominster, Mass. EDWIN A. KIRKPATRICK

The Philosophy and Psychology of Sensation. By CHARLES S. HARTSHORNE. Chicago, University of Chicago Press, 1934. Pp. xiv, 288.

In the classical tradition of psychology the sense data, the secondary qualities, are accepted as irreducible, *i.e.* as ultimate facts. Sensation being in this sense unintelligible it was meaningless to ask for explanations which went beyond the mere establishment of correlations of the type. If the eye is stimulated by light of a certain wave length, a certain color is seen; if the tongue is affected by a certain chemical solution, a certain taste is experienced; and so forth. Even the modern development of sense physiology did not in principle abandon this point of view; theories which by postulating certain chemical substances in the sense organs or other parts of the sensory sectors tried to derive the variety of our sensory experience from the physicochemistry of the nervous system did not attempt to answer the question: Why is this particular chemical reaction connected with, or accompanied by, this particular

sensation? Furthermore, sensations have affective values, and these were explained in the traditional theory as adventitious accretions, later additions to the sensations through association.

It is the outstanding merit of this book that it takes issue with this theory, that it attempts to make sensations intelligible and to understand their affective value as the core of sensory experience itself. These two problems are in reality one: "The fact to be explained is not, upon this assumption, why red, in the psychological sense, is warm, for that is like asking why a circle is round. The question is rather why red in the physical sense, that is, light-radiations of a certain frequency, should occasion the sense datum red and not the sense datum green or blue, and this again is identical with the question as to why such radiations should be responded to visually with that objectified warm feeling we call 'red' rather than with the opposite feeling we call 'green' " (p. 252 f.).

Thus the author has laid bare a psychological problem the importance of which the reviewer would be the last to dispute. But the author is primarily not a psychologist but a philosopher, and the psychology of sensation is for him only the cornerstone on which he erects a system of philosophy not always easy to understand, and whose connection with the problem of the intelligibility of sensations is, in the reviewer's opinion, not so close as the author tries to make it appear. In brief, the reviewer finds that he has mixed problems which might well have been kept separate. Thus to the psychologist his panpsychism, his "social view of sensation," seems not helpful at all. On the contrary, the psychologist gets the impression that the purely psychological theories of the book have suffered from too close a contact with its metaphysics. A discussion of the book which would do full justice to its undoubted merit would have to be primarily philosophical and would therefore not be in place in this journal. But philosophy apart, the reviewer felt baffled all along by the fact that the author treats sensations fundamentally just as sensations and not as qualities of (phenomenal or experienced) things. This treatment leads to statements which are palpably absurd: "So also with black and white; the former is the privation of seeing, and psychologically it will express this fact" (p. 146). "Back of our heads we see nothing, but we do not definitely apprehend this failure of vision as such; hence it is not black for us. But where we expect to see and do not, there we 'see' black" (p. 212). If this were true, the reader should "not-see" the print on this page. To a psychologist it seems almost equally amazing that the author treats his subject without any reference to the facts of brightness- and color-constancy. The reviewer would indeed be much interested to know what the author's theory would have done with these phenomena.

Despite these and many other criticisms the book should not be disregarded by psychologists; for it has made a positive contribution by breaking through a well established tradition, by raising a problem pregnant with possibilities, and by supporting its solutions by many well thought out arguments.

Smith College K. KOFFKA

Konstitution und Prozess in der Schizophrenie. By WOLF SKALWEIT. Leipzig, Georg Thieme, 1934. Pp. 88.

In this, the most ambitious Rorschach investigation in schizophrenia since publication of the *Psychodiagnostik,* Skalweit undertakes to use the test in separating out the relative rôles of constitution and of the disease process in producing the

schizophrenic picture. He applied the test to 90 chronic and 23 acute cases, retesting most of the acute ones at varying stages of the disease. The Rorschach findings are examined in the light of (a) clinical course; (b) physical type (Kretschmer) of each case. His principal findings and conclusions are that (a) in respect to most of the elements in the Rorschach response pattern his schizophrenics reveal a marked contrast with those of the healthy schizothyme, as reported in earlier studies by Munz and by Enke; (b) in the schizophrenic there has taken place not simply a quantitative exacerbation of trends constituting the schizoid personality, but a fundamental and qualitative change in the personality, attributable to the disease process (p. 33). Of great importance from the viewpoints of interpretation and diagnostic usefulness are also certain specific reported findings: the characteristic lack of intellectual method (*Suksezionstypus*); the overemphasis on wholes (W) and rare details (Dr) at the expense of the more obvious details (D); the trend in the direction of pure color (C) at the expense of movement (M) responses; the emphasis on the "confabulated whole" (DW and DrW); the poor form quality (F—); either very high or very low animal (A) percentage; very poor originals (O). Most of these characteristics of the schizophrenic in the Rorschach test have been noted by previous investigators although Skalweit is the first to point to the almost pathognomic value of the *Suksezionstypus*; and in giving, for the first time in the literature to this reviewer's knowledge, a satisfactory definition of this important index, he does a real service to experimenters.

The principal criticism of the investigation is the assumption, on which it proceeds, that the Rorschach test and its component indicators, are now in a sufficiently sound state of technical development to point specifically to the various processes entering into a given clinical picture, schizophrenia or other. The value of the method, as a global procedure, used by individuals of wide clinical experience and insight, has been sufficiently confirmed by various writers, to be beyond question. But this does not yet give it the status of a perfected technique. Rather the present need is for systematic experimentation, utilizing the progress that has so far been made and aimed at establishing the method on a sounder, more objective basis than at present obtains. In other words, it is, in this reviewer's estimate, still necessary to test the test itself.

Secondly, the experienced Rorschach investigator must question the grounds on which the author makes some of his psychological interpretations. Color naming is one. Skalweit considers this response the index to extreme egocentricity and most intense expression of the schizophrenic process (p. 39). From this consideration he proceeds to some far-reaching interpretations as to disease course (p. 56). Color naming and what to do with it has long been a moot point in Rorschach interpretation. Skalweit's interpretation may be the correct one, but this reviewer can cite too many examples in his own experience which can hardly be validated as indices to egocentricity. In regard to the author's findings in form quality (F+) of his patients' percepts, and their poor originals (O—), the disagreement will be not with interpretation, but with the impossibility of judging the findings themselves. Skalweit gives no example of his scoring, and in absence of any published standard set of F+ and F— norms, it is easily possible that another experimenter would find different form quality (F+) scores. There is, in a word, no such thing as controlled Rorschach experimentation at present. Exception must especially be taken to the

scoring of "absurdities" (O—) in good form quality (F+) on the ground not of the content itself, but the verbal elaboration: "die sprachliche Formulierung derselben bedingt die schlechte Orig. Antwort" (p. 23). I am wondering how many investigators score originals on this basis, and then, what is the value of the so-called "statistical" norms employed for scoring originals, whether in Zürich, Germany, or America?

The several possible minor criticisms may be omitted to leave space for noting the constructive values of the monograph. For one, the specific findings will be of inestimable value to other investigators, especially in any efforts to refine the test as an instrument for diagnosis. Secondly, there are numerous very suggestive discussions of findings, and of their possible psychological values. Color naming is one. Others are increase of the pure color response (C), reduction of fantasy life indicators (M), a new and interesting possible interpretation of M in schizophrenia (p. 27), the estimate of the affectivity-creativity balance (p. 27), and the importance of evaluating the various factors only in the light of one another and of the configuration of the entire personality (pp. 28 ff.). Implicit in these discussions are valuable inklings as to psychological structure in schizophrenia offering new leads both for speculation and experiment.

Boston Psychopathic Hospital S. J. BECK

An Introduction to Applied Psychology. By COLEMAN R. GRIFFITH. New York, Macmillan Co., 1934. Pp. vii, 661.

To be comprehensive and yet thorough in preparing a textbook of applied psychology calls for a wide range of experience and a sympathetic responsiveness to the work of individuals with widely different interests. The author shows both characteristics in abundance in this text, one which will be of benefit to instructors as well as to students. Few authors in this field would have attempted the preparation of a book for second semester students covering the panorama of subject matter presented in this book. The Webster definition of panorama—"a series of large pictures representing a continuous scene, painted on canvas that is arranged to unroll and pass before the spectator"—is an apt description of this work, if we substitute for "painted," "canvas" and "spectator" the appropriate terms. There is scarcely a psychological problem which is not touched on, making the reviewing a serious task for which the reviewer acknowledges his short-comings.

For a text the book seems at first glance too lengthy and too detailed to hold the student's interest; there is the danger that he will remain indeed a "spectator," for the student who is encouraged by concise, pithy paragraphs and classroom discussion to do further reading will be discouraged by such a complete and formidable tome. There is, unfortunately, a complete absence of illustrations, tables and topical summaries to break the evenness of the text. There should be some obligation felt to the student, of preparing the typography and contents of the book in a way which will make for readier assimilation. This has been either overlooked or conscientiously neglected.

The arrangement of topics follows a logical and practical sequence. Early chapters are headed "Psychology in Student Life," and invite early application of the material, which is on athletics, driving, rest and recreation, and motivation and study-habits. However, departure from this purpose is more than occasional; entire sec-

tions are theoretical, *e.g.* the sections dealing with problems of motivation. This difference between intention and result may be one which no conscientious writer can entirely overcome, since the problem of the applied psychologist is always that of justifying his practical recommendations by reference to theoretical "bearings." To continue, in respect to organization, the succeeding sections deal with psychology applied to social problems, law, medicine, education, industry and commerce, and art. A final section is concerned with "basic materials of applied psychology."

There is a remarkable amount of experimental material reported, which may add to the value of the book or may tend to clutter it with details, depending on how it is done. On the one hand, one feels that the author has gratified a desire for completeness, especially noticeable in the references cited on almost every page. On the other hand, one may object that the experimental material is not always properly subordinated to the more practical and perhaps less scholarly task of giving the applications of psychology to situations which the student will be likely to meet. There is considerable doubt as to the practical aspect of much of the material, and a devoted adherence to "the experimental laboratory" is likely to impress one's associates more than the student. As an example of how this attitude reacted on the reviewer, it may be said that after reading sections containing material which is experimentally sound and important he found in the section dealing with "psychological ailments" a welcomed freshness. There, and in the sections dealing with education, illustrative material is allowed some importance. There is some question as to the value of including the psychology of teaching and transfer of training as independent chapters. Could not such topics be condensed and others, such as the effect of drugs, or studies of personality by rating and schedules, be substituted? Improvement might be made in future editions by omitting several chapters, perhaps the entire last section, and by a more summary treatment of the problems of educational psychology.

The style of the writing is not brilliant, but it does show adherence to the canons of good scientific writing. Few sentences are long or involved, the diction is straightforward and not above the comprehension of sophomores. There are simple definitions where necessary, a good use of the transitional sentence, but a somewhat too frequent use of prepositions and introductory phrases, tending to make the style a little self-conscious. There are forty chapters, too many for a semester's work, and condensation here seems to have been at the expense of elaboration elsewhere. Some procedure which would have made the material more accessible to the students, especially, more frequent illustrative cases, would have aided in presentation. The index is gratifyingly complete.

University of Texas W. H. BRENTLINGER

Law and Order in Polynesia. By IAN HOGBIN. Introduction by Bronislaw Malinowski. New York, Harcourt, Brace & Co., 1934. Pp. lxxii, 296.

The author of this book spent something more than a year among the primitive inhabitants of the island of Ontong, Java, observing their manner of life, with a view to determining first hand the foundations of social order. The investigation was made under the supervision of Professor A. R. Radcliff-Brown, and the book was written while the author was working with Professor B. Malinowski. The latter has written to the book an elaborate Introduction of fifty-five pages, which almost

amounts to a piece of work in itself, and lays before the reader Malinowski's views on crime and custom in savage society. The book proper is to be regarded as rather generally confirming Malinowski's conclusions.

It should be clearly kept in mind that the conclusions are confined to the people of Ontong and to a rather cursory inquiry into information coming indirectly from some other Polynesian societies, as those of Tonga, Hawaii, and Samoa.

Getting first hand information from primitive peoples requires in the first place careful preparatory training on the part of the investigators, and this Dr. Hogbin certainly had. Then one must do the actual investigation, going among the peoples, securing an unobstructed entrance into their group, being conversant with their views of life, and securing a thoroughgoing knowledge of their customs and ways of living. This is very difficult and is only accomplished approximately. The investigator did this work with marked success, but it is the reviewer's opinion that no man reared in civilization can accomplish it perfectly. Unfortunately, until now there has been much theorizing based on fanciful reconstruction of history, or isolated statements from various localities worked into one general picture, according to this investigator. Until recently law has been looked upon as having gradually emerged from a primal communal state, in which the individual's duties were regulated by the group automatically. It has been claimed that primitive peoples therefore have no law, and some theorists have claimed to have discovered a "pre-law" stage. According to the author these assumptions are wrong, for he insists that primitive peoples do have 'laws,' if we avoid using too narrow a conception of the term.

Further, such a denial of the existence of laws in primitive societies has led to explaining coöperation and respect for right in such ways as by (1) the domination of primitive peoples by religion and magic, (2) the slavish adherence to custom consequent upon fear of the supernatural, (3) the influence of collective consciousness as practically dissolving the individual personality, *i.e.* the "dogma of automatic submission to law." Malinowski has assumed the complex interaction of individuals on one another, or the reciprocal give-and-take in which "social ambition re-inforces self-interest." This last is the tentative assumption of the author, who seeks in his study to find the answer in terms of *yes* or *no*.

He proceeds to examine such features as organization of social groups, belief in spirits, tribal ceremonies, the legal system, and then endeavors to add evidence from the other Polynesian societies.

The principle of mutual coöperation, of give-and-take, is found upon examination of human relationships in economic activities, marriage, family, kinship systems, property, and leadership. Magic, the belief in spirits, ceremonies, are all permeated by the principle of reciprocity. The spirits are supposed to know if one individual is withholding from another some dues, and the punishment is usually forthcoming, although sometimes the nemesis is not complete. Ceremonies give explicit sanction to the principle of reciprocity. Custom does the same. The same mechanism, coöperation, works in all phases of life in Ontong, Java, and in all Polynesia so far examined by the present method of the author and others. And all organized societies, shall we say, are like these, held together by this basis of law and order, interrelation of parts.

University of Denver THOMAS R. GARTH

Building Personality. By A. GORDON MELVIN. New York, John Day Co., 1934. Pp. viii, 303.

The author attempts to integrate the various viewpoints of the conflicting schools of psychology, to show that psychology to be practical must again become philosophical, and to rescue humanity from the errors of the "priests of psychology" who are today "within the temple" of the profession. He seeks to do this by explaining the human personality in terms of the life force or . . . "the true I which sits on the throne of personality" and . . . "holds sway over the total functioning of the phenomenon" (p. 92). He shows how the personality functions through the different processes of personality: the process of controlling or intelligence; of knowing, which is possible because of sensing, perceiving and naming; of habit forming; of stimulating, which he interprets as emotional activity; of purposing; of integrating; and certain other processes which he regards as explainable personality factors. He then describes the personality in action, and 'explains' how his theory harmonizes the conflicts of the various schools of psychology. A detailed outline, developed by Mr. George O'Neil for use in studying personality in the Windward School of White Plains, N.Y., is included.

Psychology, Melvin thinks, faces many difficulties which keep it from becoming an effective practical science. The many schools of psychology present conflicting views. There are dangers in holding the eclectic viewpoint because of the many different terms used by the writers of the different schools. There is confusion because of the large number of courses offered in the colleges. An added problem results, he says, from the fact that faculty psychology is still largely taught in the high schools and colleges, an erroneous view which even a cursory examination of the introductory texts written in the field of general psychology within the last decade would have corrected. He thinks, too, that it is fallacious for modern psychologists to regard psychology as a pure science. His own theory of personality, however, is dualistic. He regards personality as a life force that uses the mechanisms of the body to realize its purposes. For example, it uses the stomach to digest food, the feet to walk, and so on.

The author's effort to apply philosophy to psychology has resulted in a theoretical view that is not susceptible of scientific verification. He has developed a system of descriptive definitions which gives an inaccurate and wholly inadequate explanation of human behavior and adjustment. The following naïve explanations are typical: Intelligence controls the patterns of the personality . . . "somewhat as a driver reins in many horses" (p. 114). "By virtue of memory experience is congealed into personality" (p. 185).

Such a book can only confuse the uninformed in regard to the real problems of personality and its development. It will, however, serve to remind psychologists of their responsibility not only for discovering the facts in regard to human behavior, but also for disseminating the knowledge established by research in this field.

Georgia State College for Women EURI BELLE BOLTON

Adolescent Psychology. By EDMUND S. CONKLIN. New York, Henry Holt & Co., 1935. Pp. ix, 437.

The author points out that any line separating adolescence from other periods of life will be an arbitrary one, and thinks it is better to concentrate upon the nature of adolescent behavior than to be too anxious about sharp delimitations of its

range in the life cycle. He regards adolescence above all as "a period of social adjustment, one in which the social situation has perhaps a larger influence upon the personality pattern than at any other time in life." This represents the emphasis of the text. The topics considered are the nature and characteristics of adolescence, physical maturation and its effects, sex differences, adolescent interests and ideals, social adjustments and conflicts, family influences, romantic love, delinquency, and personality abnormalities. The volume reflects the author's own investigations of problems related to adjustments and conflicts of children and adolescents. The literature includes references to materials published each year since the great work of Hall in 1904. The materials have been rather carefully selected, although the reference citations are, in many cases, incomplete and there is a paucity of references to recent materials. The inclusion of the vast number of research studies has not detracted from the value or interest of the book, but the lack of statistical and quantitative materials in the volume is unfortunate. There are no growth curves presented from the vast array of data on growth, and no statistical data are given on the extent of juvenile delinquency such as might be found in *Juvenile Court Statistics* published by the United States Department of Labor. There is a contrast between the quantitative treatment as presented by Brooks at one extreme and the discussional and, to an extent, theoretical presentation by Conklin. Although no graphs or tabular materials are presented on physical and mental development, twelve pages (343-355) are given over to general theories of development. Though four theories of love are discussed rather fully the specific problems of present day adolescent love behavior and sex drives are largely omitted except as they relate to abnormalities of personality or delinquency. Nearly seven pages are given over to some psychological theories of the nature of conversion.

Mental development, motivation and learning are neglected while adjustments and conflicts as related to present day social problems and home influences seem to be relatively overstressed. The terms "family consciousness," "fixation," "emancipation," and "psychological weaning" (chs. 12 and 14) are used quite freely in discussing the influences of the family upon the growing child. This represents, in a measure, the psychoanalytic approach made in this volume to the study of the adolescent.

An interpretation of specific statements apart from the context is likely to be misleading, but the following statements as interpreted by the reviewer in the light of the context seem to reflect the psychological view of the author as to sexual maturation: "The maturing sex structures and functions produce new sensory experiences. How these will be perceived, what they mean to any individual boy, will depend of course upon his background of knowledge at the time" (p. 24). "With the maturation of the sex organs in pubescence, sexuality becomes definitized and focalized in the personality pattern. It is not something new, but a development into detail of that which had all along been present in a rather vague fashion." There is hardly an adequate discussion of the nature-nurture question, a question of prime importance in understanding adolescent problems. The complexity of behavior problems is well recognized as indicated in this statement: "When the possibilities of distortion and disturbance of normal growth changes are fully canvassed they are always found to be distressingly many and intricate" (p. 426).

In general the book is well organized, and the author has constantly made use of scientific materials to support his discussions. His references to the studies of

Lehman and Witty dealing with adolescent interests, and to Furfey's data, relative to social development, as well as those to the studies of Terman, are illustrations of the sort of scientific data used in supporting various ideas. The reviewer recognizes that lack of space, the interests of the author, and the aim of a text are determining factors in the choice of materials. The present volume presents an interesting and valuable approach to the study of the socialization process during adolescence. Like most other productions, it contains some minor inconsistencies in the references, but it is mere trituration to fill a review with these. From the standpoint of its usefulness as a text, it should appeal to teachers, parents and counselors who are interested in and concerned with a well organized approach to individual behavior developments of adolescents.

North Carolina State College K. C. GARRISON

Contributi del Laboratorio di Psicologia. By A. GALLI, A. GEMELLI and G. PASTORI. Milan, Pub. Catholic University of the Sacred Heart, 1935. Pp. 194.

The eighth volume in series six (Biological Sciences) of the publications of the Catholic University of the Sacred Heart contains only 4 items as compared with 19 in volume six. (The intervening volume, numbered 7, was devoted exclusively to a study by Gemelli and Pastori of the electroacoustic analysis of language.)

(1) A. Galli, Percezione totalizzatrice della forma attraverso alla fovea centrale nella luce crepuscolare.

The problem here was to study the conditions under which the figure and ground fuses under dim illumination. The author used various forms of textile patterns with controlled illumination (12 subjects). Galli concludes that to obtain fusion: (1) figure must be of intermediate clearness not contrasting with ground, (2) ornamental design must comprise a unity for the subject not by its totality but by its parts, (3) optimum distance must be determined in relation to both the illumination intensity and the visual acuity of the subject, (4) the fusion process has three phases: (a) the figure keeps its characteristics and loses its perceived objectivity, (b) figure acquires a nebulosity and transparency permitting the subjective integration with the ground, (c) degree of fusion depends upon proportion of ground covered or emerging.

(2) A. Galli, Osservazioni sulla reproduzione di profili a più significati.

E. presented his subjects (22 in one group and 12 singles) with black and white designs of varying complexity, facing right and left, both by visual and by tactile means, and required them to reproduce them as drawn designs (1800). While there is no great difference between the two modes of presentation, the author finds a great abyss between perception and capacity to reproduce. Figures are greatly altered in reproduction and only some subjects attributed significance to their reproductions (human face).

(3) A. Gemelli and G. Pastori, I metodi della elettroacustica nello studio del linguaggio.

General discussion of the uses of the oscillographic technique in the study of speech with 18 plates showing oscillographic records of various speech phenomena reprinted from vol. 7.

(4) A. Gemelli, Exercice et apprentissage.

On the basis of various theoretical considerations and experimental results on

industrial operations Gemelli formulates the following conclusions concerning exercise and learning: (1) To evaluate the influences of practice on earning one must compare subjects at beginning and end of practice, and observe just how the activities are accomplished. (2) Sheer practice (mechanical acts) must be distinguished from training which leads to the acquisition of a habit. (3) Studies must not be limited to easily automatized actions from which intelligence factors are excluded. (4) Exercise must not be studied as pure mechanical repetition, but as invoking actions which are to constitute phases of a given operation. (5) Simple repetition of an action does not improve it, but rather the way the individual performs it. (6) In instructing learners mechanical repetition must give way to a betterment of performance in order to arrive at a given end.

The editor of the Contributi is to be congratulated on a vast improvement this issue shows in format, paper, and general appearance.

Indiana University J. R. KANTOR

The Psychology of Learning: A Textbook in Educational Psychology. By ROBERT A. DAVIS. New York, McGraw-Hill, 1935. Pp. xii, 489.

In his Foreward Jordan enumerates the outstanding merits of this book. It is catholic, introducing its readers to a great many modern experimental studies and to most of the more important theories. It attempts to encourage critical examination by the student. Finally, the author, without as a rule attempting to resolve the conflicting views, indicates the practical implications and applications possible in view of the known facts and the current state of theory.

These are virtues and the reviewer turned the pages with some eagerness for he needs just such a textbook. It is with keen regret that the verdict must be unfavorable. The eclecticism tends again and again to slip into mere syncretism—just the assemblage of facts and theories without integration. (This is a very different thing from true eclecticism which is far from being a wishy-washy doctrine that there is a bit of good in all theories. Eclecticism takes only those items from current theories which can be harmonized and is thus a system by itself.)

An associated weakness is the failure to carry through. For example, in the chapter dealing with perception there is a brief section on illusions, summarizing experiments by Judd and by Lewis on the Muller-Lyer (*sic*) figure. The conclusion that improvement in resisting illusions is mainly a matter of perceptual technique is not followed up. The whole passage about illusions is thus left without significant relation to other kinds of perceiving. The reviewer does not press for a generalization from illusion to perception in general which is not valid; but if the author sees none, the section on illusion has no place in this chapter. The main objection is to the way in which the book is written. A textbook, of course, is worthless if not scholarly; there is no wish here to disparage the book on that score. The passing of this necessary first test, however, does not justify a book whose function is pedagogical. If a textbook cannot be brilliant and sparkling, it should at least be clear and reasonably fluent. This book is neither; it is slovenly.

Such a charge requires proof. Here are a few examples which are typical of what may be found on page after page. First, there is what Fowler calls the effort after elegant variation. A man is "responsible for" an investigation when he really carried it out himself. Sometimes this locution leads to ambiguity as when Stern is said to be "responsible" for many investigations of testimony. Is it neces-

sary to labor the point that when a term is misued even in places where it cannot be misunderstood, we ruin it for use in other contexts where it is required in its proper significance? Elegant variation in construction may turn sense into nonsense. Thus "the first two factors may be objectively measured while the third affords much opportunity for subjective estimation" (p. 62). The context shows that the author must mean that the third *depends* largely on subjective estimation; he says something quite different. Again we have, "It is pertinent to question the validity of a *process* so simple, although most of the current explanation *principles* have been based on this *concept*. The *point of view* has been challenged by Lashley and others yet no *principle* has been set forth which is supported by sufficient experimentation to warrant a different *approach*" (p. 76, italics the reviewer's). Process = concept = point of view = principle = approach.

For carelessness, let these stand as exhibits: "This view confirms the findings of Haught" (p. 44). Findings usually confirm views, not the reverse. Are we not actually encouraging the students in thinking unscientifically when we write like that? "Reports . . . are seldom free from error. This condition . . . exists among laymen." The condition of freedom from error? Again, "The difficulty lies in the explanation of the functioning of the C.N.S. about which several theories have been formulated" (p. 76). There are four possible antecedents of 'which.' Finally, consider this sentence: "The process by which substitute stimuli produce the same reactions as original stimuli has elicited the conditioned reflex as an explanation for association." Why *elicit?* Because stimuli have commonly been said to elicit reflexes? But the stimuli are not here the subject of elicit and the object of elicit is not really a conditioned reflex but the conditioned reflex hypothesis. What is actually asserted, however, is that the conditioning process has elicited the conditioned reflex and this reflex (as a concrete fact of behavior) is an explanation of association. Which is sheer nonsense.

We may tolerate such writing in a research study the justification of which does not lie in its manner but in its matter, but can we tolerate it in a book which must be justified on the basis that it conveys ideas accurately and readily? The author has got the makings of a fairly useful book. But he has not written it yet.

Ohio State University HORACE B. ENGLISH

Pacemakers in Relation to Aspects of Behavior. By HUDSON HOAGLAND. New York, Macmillan Co., 1935. Pp. x, 138.

"The point of view which we wish to stress is that much of the overt behavior of organisms is determined by the interrelations of chemical events within cells and groups of cells, quite independently of external environmental factors." This quotation states the thesis which Dr. Hoagland's monograph is defending. The details of the argument supporting this thesis are not unfamiliar to psychologists, having been presented as part of Chapter I (pp. 59 ff.) of the *Handbook of General Experimental Psychology*. The present work includes a great deal more material concerning the dynamic steady state, particularly Osterhout's work on large plant cells, Hoagland's studies of lateral-line reception in fishes, and a discussion of peripheral inhibition or adaptation especially in mechanoreception. There can be little doubt as to the importance of chemical "pacemakers" in the physiological functions of organisms.

The value of this contribution to psychology is, however, another matter. This issue is not of the reviewer's making, but of the author's. Over half of the introduc-

tion is a criticism of behavioristic empiricism, which surely enough needs criticism, but probably not on the author's grounds. In the last chapter he says, "The conception of the pacemaker control of conduct has a degree of specificity . . . which is better adapted to experimental purposes as an hypothesis than . . . the Gestalt conception of experimental psychology." Again he says that certain "problems of traditional interest to psychologists" are "usually interpreted by them in terms devoid of physicochemical meaning." The meaning here is sufficiently clear—psychological data must be interpreted in terms of physical chemistry, but this is a manifest impossibility.

Learning is without doubt a major interest of psychologists. Many people have attempted biochemical interpretations of learning phenomena, but the net result has been exceedingly small. Hoagland reported in the *Handbook of General Experimental Psychology* that simple maze learning in ants was apparently speeded up by increase in temperature suggesting a chemical facilitation of learning. This may be true in cold-blooded animals, but it does little to solve the problems of learning in birds or mammals where significant changes in body temperature are found only in infectious or artificially produced fevers.

Hoagland's demonstration of the importance of the K_I/K_o ratio in sensory adaptation is of great importance, but this problem is generally recognized as a physiological not a psychological one. The laborer working with caustic soda certainly exhibits sensory adaptation, yet the psychological interest in why the man stays on such a job is only incidentally concerned with this factor.

Psychology must stand or fall on its own postulates and its own data. It cannot risk dependence upon physiological principles which even in their own field may not be any too well established. Therefore, as physiology this monograph appears to be a valuable contribution (if the present reviewer may be allowed the privilege of evaluating it). When, however, the author attempts in a somewhat artificially forced manner to insist that this material is of vital importance to psychology, the psychologist must ask for a more convincing demonstration.

Indiana University C. M. LOUTTIT

Individual Differences: The Nature and Causes of Variations in Intelligence and Special Abilities. By FRANK S. FREEMAN. New York, Henry Holt & Co., 1934. Pp. xi, 355.

Heretofore individual differences have usually been treated in brief chapters in more extensive works. The purpose of this book is to bring together the results of the many independent studies and experimental investigations and to integrate the subject matter into one volume. Consideration is given to the higher mental processes only.

The author shows that the popular tendency to classify individuals into one of two extremes of a particular characteristic is an error since all characteristics tend to follow the normal probability curve and there is continual gradation from one extreme to the other with much overlapping in different traits.

The treatment rests upon a critical evaluation of the major investigations from which conclusions are drawn according to the weight of the evidence. While the data themselves are not new the treatment permits the author to present an analytical statement of results. A particularly compelling interpretation is given to the comparative value of heredity and environment in the development of individual differ-

ences. It is shown that nature and nurture cannot be separated and that the development of innate abilities must conform to an articulated refinement of the environment, wherefore the probable limits of future performance is unknowable. All that the author states here is true but in his conclusions he seems to have made no allowance for the fact that it is apparently impossible to measure potential ability without using knowledge acquired through training. In discussing race differences the author points out that "the differences within a national or 'racial' group are enormously greater than the differences between the averages of any two national groups" (p. 184). The last part of the quoted sentence is apparently meant to include also racial groups. Between the Negro race and white 'races' the author recognizes that "innate mental potentialities and manifested intelligence" are not to be confounded. Yet he concludes that the Negro and Indian races are inferior to the white race in "intellect," with the reservation that it has not been demonstrated that the differences are due to genetic constituion. However, it is impossible to say finally that there are real race differences in intelligence. Some confusion results from the failure of the author to make clear his conception of intelligence, which at times seems to mean 'native capacity' and again in other connections to be a synthesis of education and native capacity. The adequacy of the tests used is not discussed. This is especially unfortunate in race comparison since the validity of a test standardized upon the cultural background of one race to measure the "native capacity" of a different race is doubtful.

Age, sex, and special abilities are also treated critically, with the result that there would seem to be less certainty today than there was some twenty years ago as to the cause, extent, or even existence of individual differences.

The subject matter is logically presented. The abundant citations of source material—a nine-page supplementary bibliography in addition to frequent footnote citations—and the careful and objective treatment make the book a valuable addition to psychological literature.

Peabody College H. W. FENKER

Handedness: Right and Left. By IRA S. WILE. Boston, Lothrop, Lee & Shepard Co., 1934. Pp. xiii, 439.

The content of this book is largely an historical rendition of the problem of handedness. Opinions of men from the time of Aristotle to the present are given on such diverse topics as religion and hand symbolism, magic and hand values, the luck idea and hand preference, heliocentrism and handedness, philology and the hands, and the hand preference of primitive man.

Throughout the book Wile emphasizes the interdependent rôles of sun-worship and primitive warfare in the phylogenesis of handedness. For example, "It is probable that man's persistent organic leaning movement towards the sun, his daily expectation and greeting of the sun, and his following of the course of the sun to his right in the northern hemisphere, in which he probably originated, were all responsible for the accentuation of the right hand as a significant factor in his own behavior and his own movements." Again, "possibly dominant handedness has a real survival value. Certainly if man was ambidextrous when he rose in his new majesty to the erect position, he had a new hazard to face in that there was a more direct exposure of his vital organs to his enemies, the wild animals and his hostile neighbors. The early ambidextrous man would soon see and understand that there was

greater safety in protecting his left side and in turning his right side toward his foes."

Handedness in modern man is considered an inherited characteristic, left-handedness being a recessive Mendelian trait. This assertion is feebly supported by the observation that about 25% of the population carry packages and umbrellas under the left arm. It is contended, however, that social forces tend to mask hereditary left-handedness in most manual performances.

In a final, brief chapter the argument is advanced that "natural handedness should be fostered and conserved" in order to prevent "nervous tensions." Stuttering, reading disabilities, and social maladjustments frequently are coincident with, if not caused by, a shift in lateral preference. No reference is made to the experimental literature in order to substantiate these claims. It seems unfortunate that a book on handedness which will be read by persons interested in scientific topics (this volume is recommended by the Scientific Book Club) should contain no mention of the extensive and authoritative work of Travis, Dearborn, Monroe, and others.

University of Oregon LESTER F. BECK

Educational Psychology. By A. M. JORDAN. Revised ed. New York, Henry Holt & Co., 1933. Pp. xvii, 522.

This revised edition, following the same general outline and point of view as found in the first edition (1928), embodies such changes and new material, with references, as certain recent researches in the field have made necessary. There is more recognition than formerly of the possibilities of transfer of training under conditions of instructions emphasizing principles and similarities of materials or functions. The older view of "identical elements" does not hold "in the psychology of today" (p. 217).

Two new chapters "Maturity and Growth" (Ch. 11) and "Measurement of Personality Traits" (Ch. 16) appear. The first of these summarizes more fully than formerly data (mainly from Baldwin and Stecher in the early '20s or from earlier sources) on growth in height and weight, also data on growth in emotions and interests (chiefly in reading) and in intelligence. Strong's studies of interests are not referred to, and nothing quantitative or definite is given, in this connection, on emotional growth. The data on growth of intelligence and ability to learn are mainly shown in curves, representing different conceptions, including Thorndike's curve of ability to learn in relation to age. No definite position as to what units to use on mental growth curves is taken. IQs from such group tests as the N.I.T. scores are used as if they were as valid as Stanford-Binet IQs. The lack of data from successive tests on the same individuals is not sufficiently noted and cautions as to improper samplings of different *Ss* at different ages are neglected. This is not unusual, however. Various types of personality measures are clearly presented in Chapter 16 with illustrative material from the sources, and reliabilities and validities are indicated. The chief topics are ratings, questionnaires, and performance tests. Precautions against errors are indicated but there is no general, critical consideration or evaluation of the meaning or nature of "traits" as thus measured.

The new edition maintains the many good qualities of the first edition, such as, clearness of presentation and thoughtful organization of material. *A Manual of Experiments in Educational Psychology* is promised for this revision of the text.

The author does not assume that it is necessary to give detailed instruction in the way of how to teach this and that subject matter, but emphasizes scientific methods.

J. P.

Readings in Industrial Psychology. By B. V. MOORE, and G. W. HARTMANN. New York, D. Appleton & Co., 1931. Pp. xxxix, 560.

In the face of the difficult task of constructing a volume of readings in a heterogeneous field the authors have acquitted themselves in a fashion which might well attract the attention of those who plan similar books in other fields. In general, the editors of such volumes have sinned in at least one of two respects. Either they have given citations so out of context as to be meaningless, or they have prejudiced the reader by the narrow range of quotation or by interlarded comment. Moore and Hartmann avoid both of these errors of commission. One is constantly amazed at the skill they have shown in abstracting quotations which really provide well-rounded samples of the original writers. Their own comments are brief and seldom dogmatic. Most of the quotations are presented without comment (save an occasional damning subtitle) and on controversial matters of importance, the several points of view are properly represented.

There are in all eighteen chapters which carry the reader from a general introduction though vocational guidance and selection, 'scientific management' and its allied psychological studies of the conditions of increased output, to later chapters on accidents, monotony, motivation, social adjustments and difficulties, concluding with a brief treatment of marketing the product. An initial section gives brief professional histories of the more important figures in industrial psychology which should serve to give the uninitiated reader a means of judging the professional standing of the men from whom quotations are taken. The more important contributors in each field are well represented, and, in the main, their statements are not 'adapted' or tampered with in any way. The British publications are generously sampled and there are even a few brief items from the German journals. One might wish that the latter were given better representation (Moede, for example, appears but once in these pages). It is to be regretted, too, that the writers have failed to emphasize the fact that the best book of readings ever written is no substitute for first-hand knowledge of primary sources. These faults fail to loom large, however, in a volume which reveals general signs of careful editing and wide acquaintance with the literature. It is to be recommended to those who may want an outline of the problems and techniques in this particular phase of the applications of psychology.

Cornell University JOHN G. JENKINS

A Critical Introduction to Ethics. By PHILIP WHEELWRIGHT. New York, Doubleday, Doran & Co., 1935. Pp. xvi, 463.

As a textbook in ethics this volume provides the student with a fresh and critical discussion of the traditional materials (Hedonism, Utilitarianism, Rationalism, etc.) as well as an exceedingly stimulating introduction to advanced thinking concerning present-day social problems. The psychologist, however, will be disappointed with the author's lack of acquaintance with recent developments in general and social psychology and their implications for ethics.

According to the author ethics sustains a double relationship with psychology.

First, every moral evaluation or decision as a psycho-physical activity is a fact in some individual's mental life. But although the author states that of all the sciences psychology has the most intimate connection with ethics, that connection is really no closer than the relationship of ethics with chemistry or biology. For the moral fact is indeterminate—an expression of value, a normative or metaphysical, not a descriptive phenomenon.

But this relationship refers only to scientific psychology. "If it (psychology) gives us a knowledge of human nature in the concrete, as much everyday conversation and honest literature tend to do, then the study of psychology carries with it an insight into moral values." The author believes that through imagination one can stretch out to the most indeterminate realms of self-transcendence. This conception concerning psychology and ethics fits in with the author's notion that ethics is dialectic and not subject to the restrictions of natural science.

Indiana University J. R. KANTOR

Erotik und Schaffen. SIEGFRIED PLACZEK. Berlin, Marcus & Weber, 1934. Pp. 226.

This volume is a discussion of the relationship of creative art to sexual mechanisms. The book is rather nondescript in that some chapters consist of discussions with little reference to specific cases while others are devoted to individual examples; nevertheless, the usual psychoanalytical arrangement is found in it for it begins with a discussion of creative and sexual relationships in the Greek mythical period, and then takes up modern sexuality. The second two chapters are largely essays, one on chastity and the other on sadism and masochism in relationship to creative art. These are not long chapters and agree with the accepted psychoanalytic ideas on the subject that in these particular spheres of sexual abnormalities there are distinct but not distinctly analyzable relationships to art. The remainder of the book is occupied by two chapters which deal with biographical material, and a final concluding chapter. One of these biographical chapters stresses homosexuality including studies of Oscar Wilde, H. C. Anderson, Walt Whitman and others, none of whom are clear-cut cases. The chapter on heterosexuality treats of three different groups of biographies—writers, musicians, and painters. The author makes a rather clear-cut case of the analyses of the numerous individuals whom he selected; but in spite of all his analyses of writings, biographies and other second-hand information, the thesis is not conclusive.

The book may be of interest to persons who are concerned with the motives behind art as interpreted by rather "romantic" analytic methods; it has little of value to psychologists. The examples at times seem far fetched and, in many cases other studies of the same personages have not agreed with the present author's findings. As far as the general thesis is concerned, that creative art and sexuality have a relationship, this has been the belief of students of art for generations.

Wayne University LOWELL S. SELLING

Geistesformung: Beiträge zur experimentellen Erforschung der formalen Bildung. By JAIME CASTIELLO. Berlin and Bonn, Ferd. Dümmler, 1934. Pp. 142.

The results of the author's investigations of various types of school curricula upon the students' reactions to general questions in various fields are reported and arguments advanced in favor of transfer of training, within limits laid down by the findings. Even manual training was found to improve habits of work, tidiness, and

general attitude. The volume contains excellent accounts of previous work done in the field, critically assessed, and organized under appropriate headings. While the 'experiments' upon which the author's arguments rest were mostly of the paper and pencil type they represent an attempt to answer definite questions, clearly and philosophically conceived. One accustomed to the sort of thing done in England and this country on questions lying on the borders of education and psychology will find this volume interesting and novel even though it may not meet the strictest canons of scientific experimentation.

Bryn Mawr College HARRY HELSON

An Attempt to Predict the Success of University Freshmen in Their Adjustment to Scholastic Work. By OMAR C. HELD. Ann Arbor, Mich., Edwards Brothers, Inc., 1933. Pp. vii, 50.

This study develops simple regression equations for the prediction of academic success (as based upon the University of Pittsburgh quality point scale) from scores on Thorndike Psychological Examination, Thorndike Reading Test, High School Index, Composite Placement Test, Cross English Placement Test, Foreign Language Placement Test, and freshman grades. The multiple regression equation is derived for the three factors, Thorndike Psychological Examination, High School Index, and Composite Placement Test. The study also treats of the relation between certain non-academic factors, as occupation of father, health, sex, etc., and academic success during the freshman year.

The title is misleading since it would suggest that actual predictions had been made and that any variation between predicted and actual performance would be discussed.

George Peabody College H. W. FENKER

Hygiene of the Mind. By ERNST VON FEUCHTERSLEBEN. Tr. from 3rd German ed. by F. C. Sumner. Introduction by Esther Loring Richards and preface by the translator. New York, Macmillan Co., 1933. Pp. xviii, 150.

This book will make the naïve reader realize that mental hygiene was practiced by intelligent physicians and clergymen long before Clifford Beers organized the mental hygiene movement in 1908. It is, however, not suitable either as a guide to mental health or as a textbook for classes in mental hygiene. Only the informed reader will be able to sift that which is valuable from purely unverified speculations.

The author has a modern viewpoint in that he stresses keeping people healthy rather than curing those who are mentally maladjusted. His best discussion is perhaps on "phantasy," a combination of suggestion, hallucinations, and delusions. He discusses with insight how "phantasy" has caused much mental and physical ill health, and points out rather clearly how it could be used with equal success for maintaining health as well as for curing those who are ill. His statement that health, mental and physical, "is nothing other than beauty, goodness and truth," is of course not original.

Ball State Teachers College L. L. HYDLE
Muncie, Indiana.

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